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BRAITHWAITE'S RETROSPECT.

VOL. LXXXI. JANUARY—JUNE, 1880.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

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AN ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, WITH OTHER SHORT ARTICLES FROM THE MEDICAL JOURNALS, SHOWING THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS DURING THE HALF-YEAR. ARRANGED ALPHABETICALLY.

AFFECTIONS OF THE SYSTEM GENERALLY.

ABNORMALLY HIGH TEMPERATURES.—The following points seem now to be clearly established: 1. Temperatures above the degree formerly supposed to be necessarily fatal do sometimes occur without a fatal issue; nay, even without extreme peril to life. 2. Such exceptional and excessive temperatures as a rule end in recovery. 3. The conditions of body in these cases of excessive temperature appear to be distinct from the conditions existing in fevers, in which the rule as to the extreme peril of temperatures of 107° and upwards remains unassailed. (Mr. J. W. Teale, p. 39.)

ACUTE RHEUMATISM.—The treatment of the disease by rest, by a comfortable bed, by good hygienic measures, by careful nursing, by local and general anodynes, is apparently all that can be done. Notwithstanding what has been advanced to the contrary, so far as our hospital statistics dealt with the matter, no specific cure for rheumatism appeared to exist, while all drugs hitherto administered—salicylic acid and its congeners not excepted—seemed palliative rather than curative in their action. (Dr. R. Southey, p. 67.)

AGUE.—*The True Cause of.*—Professors Klebs and Tommasi-Crudeli report the particulars of a research recently conducted by them at Rome with regard to the essential cause of the ague, which is endemic about that city; and, so far as can be judged from that account, the doctrine that ague-poison is a microphyte of malarious soil is no longer a mere matter of suspicion (as my text expresses it) but is a matter of experimental certainty. The investigators, namely, declare that they can isolate from malarious soils and their atmosphere definite microphytic forms capable of separate cultivation; and that, when successive generations of this "bacillus malariæ" (as they name it) have been cultivated in successive

quantities of an indifferent fluid, subcutaneous inoculation of rabbits with any final fluid in which the *bacillus* is germinating will give ague to the subjects of the experiment. (Mr. J. Simon, British Medical Journal, Dec. 20, p. 975.)

CANCER OF THE WOMB.—Prof. Clay, of Birmingham, strongly recommends Chian turpentine in cancer. In one case, he says, I prescribed Chian turpentine, six grains; flowers of sulphur, four grains; to be made into two pills, to be taken every four hours. No opiates were prescribed or lotion used. No change was to be made in her diet or occupation. On the fourth day after taking the medicine the patient reported herself greatly relieved from pain, and was in better spirits, but she complained of a large amount of discharge. Twelfth week, my notes are—the parts feel ragged and uneven, and do not bleed on roughly touching them. The speculum shows several cicatricial spots. The turpentine has been taken regularly during the day for twelve weeks every four hours, during which time she has been almost free from pain and has had no hemorrhage; no glandular enlargement; general health improved. Walks easily to the hospital, being about a mile distant. The following formula was ordered in another case:—Solution of Chian turpentine, half an ounce; solution of tragacanth, four ounces; syrup, one ounce; flowers of sulphur, forty grains; water to sixteen ounces: one ounce three times daily. This form of mixture was given to the patient, and was much liked. She has now taken the turpentine for thirteen weeks uninterruptedly. The os uteri is a little more than one inch in diameter, and feels like a ring of cartilage about a quarter of an inch in thickness. The tumour has nearly disappeared, and the finger can be introduced posteriorly into the uterus for more than an inch. The general health has much improved, and she is quite free from pain and looks cheerful, and is becoming stouter. No sedative whatever has been given during the treatment. It is a most efficient anodyne, causing an entire cessation of pain in a few days, and far more effectually than any sedative that I have ever given. In the cases I have described no sedative was employed in any instance, although in some cases where great pain had existed previously to commencing the treatment, large doses had been given. All the patients after several months' treatment are living, and the disease has not advanced as is usually the case, but has retrogressed—in fact, has all but disappeared; and it may at least be safely asserted that when the remedy is steadily used for some time it arrests the progress of the disease, and relieves the pain incidental to the morbid growth in a manner which cannot be said of any other remedy. (Prof. John Clay, p. 58.)

It is by no means easy to obtain Chian, or Cyprus turpentine. So long ago as the date of the publication of the earlier editions of Pereira's Materia Medica there was so much difficulty in procuring the substance that the profession was especially warned against the almost inevitable substitution of Venice or Canada turpentine, or some other terebinth of totally different properties, in dispensing of prescriptions, for Chian turpentine. Probably there is scarcely any of the true resin in the market at present, and only druggists who happen to possess a small forgotten store *can* supply it. We think it desirable to make this intimation for the sake of medical practitioners who may be anxious to try the remedy, but who are almost sure to be disappointed, unless they take more than ordinary measures to ensure accuracy. As Professor Clay stated in his paper, no other terebinth except the Chian has been known, or can be expected, to produce the effects which have followed its use in his cases. (Editor of Lancet, p. 66.)

INFECTIVE AND CONTAGIOUS DISEASES.—It is generally agreed amongst pathologists to separate the true specific contagious diseases from such diseases as pyæmia, septicæmia, and traumatic erysipelas,—from which they are distinguished by several peculiarities. These are, that the specific contagia are not known to arise *de novo;* cases always arise by transmission of the virus from a previous case. The diseases are, within certain limits, "like the father that begat them"—have a similar course, similar symptoms and morbid processes, and reproduce similar cases, which, however modified or lessened by individual proclivity, are always liable to revert to the original type of severity. Moreover, the virus is often specially limited to, or most intense in, some particular tissue or fluid; often that which forms the specific lesion—as the vesicle in vaccinia, the eruption or nodule in glanders,—though not absolutely confined to that part. The other class, though they may be specifically contagious or inoculable, as some forms of septicæmia, erysipelas, and perhaps diphtheria, can be originated without infection from a previous case, and very commonly arise in this way. They have no period of latency or incubation except such as results from the time needful for entry of the poison into the system, and no specific lesion at all comparable to that of the acute specific diseases. Moreover, they not only usually originate in processes of the nature of decomposition, but their tendency is to cause decomposition of the tissues and fluids, both during life and rapidly after death. They arise at a certain spot, usually in wounds, and thence infect the system, and hence are distinguished as *infective*. They are

usually readily intercommunicable by inoculation to widely different classes of animals, and are not strictly limited to certain classes, as is generally the case with specific infections. (Dr. W. S. Greenfield, p. 27.)

PYÆMIA.—The symptoms of pyæmia are those of enteric or typhus fever, with a large but varying intermixture of the hectic form. The main distinctive criteria are the following:—If the rigors and the perspirations which commonly signalise the onset are repeated throughout the career of the case in disorderly fashion, you may safely exclude enteric fever, typhus, ague, and acute rheumatism. Moreover, in the first-mentioned the fever-spots are pathognomonic and cannot be mistaken for the sudamina and pustules of pyæmia. Far above and beyond all other criteria is the testimony of the thermometer. The free and fitful oscillations on the chart are unerring and all-sufficient guides to the diagnosis. Nevertheless, it would be well to pause awhile and to enlarge upon the differential diagnosis of pyæmia and acute rheumatism. No two diseases are so often confounded; indeed, at the first sight the confusion is almost inevitable unless there is something in the circumstances of the case positively to foreshadow the coming of pyæmia. I warn you now in all cases that bear the least resemblance to acute rheumatism, when associated with nervous derangement, never on any account to allow the temperature to go beyond 104°, or at most 105°, before you resort to the bath. (Dr. Henry Thompson, p. 55.)

TYPHOID FEVER.—From the first the patient should be restricted to liquid diet, with farinaceous food, and bread in some form if the appetite requires it. It is better to vary the broths, and to add to them some strong essence of vegetables. The value of milk as an article of diet in fever is generally admitted, but it requires to be given with caution. The indiscriminate employment of milk in almost unlimited quantities as diet in fever has led to serious troubles. Milk contains a large amount of solid animal food. The casein of the milk has to pass into a solid form before digestion can take place. Curds form in the stomach, and, the digestive powers being weakened in fever, these curds may remain unchanged in the stomach, and produce considerable disturbance of system. Sleeplessness is a more important symptom, for although it also usually disappears or diminishes spontaneously during the second week of disease, still this is not invariably the case; and the nervous system may be greatly worn by the want of sleep, lasting as it does occasionally, unless treated, night and day. The drugs I employ when,

from the continued sleeplessness, it seems absolutely necessary to relieve it, are henbane, bromide of potassium, and chloral. From a combination of the latter I have seen very good results, and, so far as my experience has gone, no really ill effects when its use has been limited to the earlier stages of the disease—*i.e.*, to the period anterior to signs of nervous prostration. If the patient's temperature be high, a tepid bath, or sponging the surface will often at once induce sleep, and no drug be required. In the earlier and also in the later stages of typhoid fever the sleeplessness has been treated by opiates. Experience has convinced me that although, in some cases, opium in sufficient dose to secure sleep has afforded relief, it is on the whole a most dangerous remedy. When the stools are strongly alkaline, diluted sulphuric acid sometimes affords marked relief. When the stools are merely frequent four ounces of starch water thrown into the rectum night and morning will often check the frequent action. Should this not prove efficacious, from three to ten drops of laudanum in one ounce and a half of starch water may be thrown into the bowel night and morning *after* the passage of the stool. Carbonate of bismuth, in twenty-grain doses every four or six hours, is one of the best remedies I know for the catarrhal inflammation of the bowel itself. (Sir W. Jenner, p. 11.)

Cold Baths in Typhoid Fever.—The treatment of typhoid fever by cold baths, when the temperature reaches 104°, or even less, is very generally adopted in Germany; but neither my own limited experience nor the evidence adduced by others in its favour has carried conviction to my mind of its advantage. At the same time I entertain no doubt that the direct cooling of the body is in some cases essential to the preservation of the life of the patient—gives him, so to say, his only chance of recovery; while in others it alleviates the severity of a symptom which increases the danger of the patient. A certain degree of elevation of the body is incompatible with the favourable progress of the case. Thus a temperature of 106°, rising to 107°, and still advancing, will, unless lowered by treatment, speedily end the life of the patient; for these fortunately very rare cases the direct and rapid depression of the patient's temperature by the cold bath is essential—is the only source of hope. Again, a temperature of 105°, if continuous for forty-eight hours, or broken only by a slight fall of short duration, may itself, by the degenerative changes it favours or produces in the heart and other organs, cause the fatal termination; for these cases a tepid bath gradually cooled down is sufficient to

bring down the temperature; the bath has to be repeated as the temperature rises. (Sir W. Jenner, p. 17.)

In the administration of our baths we had regard less to the temperature than to the age, the state of heart, restlessness, and sleeplessness, and chiefly to the last. If the patient had not slept for two or three days, even if the temperature fell short of 103°, he was wrapped in a flannel dressing-gown, and placed in a bath of 70° Fahr., gradually reduced to 60° in from ten to fifteen minutes, at the end of which time he was removed to bed, wrapped in a blanket. In nearly all the cases, we thought it prudent to give some stimulant to the patient in the bath, from noting that its effect was to weaken the pulse, which fell but a few beats in frequency, and became thready, compressible, wavered, and sometimes almost disappeared. In other words, that the bath was useful in relieving symptoms in those cases whose natural termination is recovery; inapplicable, by reason chiefly of cardiac weakness, in those cases whose natural termination is death; that is, in the cases in which specific treatment is needed. The depressing effect of the bath has been noticed by Professor Liebermeister, who considers a very high degree of cardiac weakness an important contraindication. To put the case briefly, those severe cases in which there is every probability of a fatal result from bodily weakness and severity of disease are not cases in which the "antipyretic treatment" is admissible. "If fever depend upon a poison in the blood, it is not to be dislodged by the act of vomiting, nor washed out by the forcible descent of cold water upon the skin." (Sir T. Watson.) I believe the expectant treatment to be the best. (Dr. Alex. Collie, p. 25.)

SEPTICÆMIA.—The elaborate researches of Prof. Polli, of Milan, seem to afford conclusive evidence as to the great power of sulphurous acid as an internal antiseptic, when administered in the form of sulphite of soda. He found that animals could, without any apparent ill effect, swallow and absorb large doses of the sulphites; and it was observed that when these animals were killed their bodies long resisted the putrefactive process, and that, whilst an animal killed under ordinary circumstances exhibited rapid and marked signs of putrefaction, an animal which had previously absorbed doses of the sulphites betrayed no signs of putrefaction whatever. Again, in a series of experiments in which no less than 300 dogs formed the bases of the deduction, Prof. Polli showed that the sulphites exerted a *prophylactic* and *curative* power when septic poisons were introduced into the economy. As regards the human subject, he found that the stomach would

tolerate large doses of the sulphites of soda or magnesia, which "were tried in the various eruptive fevers, intermittents, typhus, typhoid, pyæmia, puerperal fever, dissection-wounds, malarial fevers, &c. The records of cases treated in this way showed an extraordinary amount of success." According to Ringer, moreover, "so powerful is the antiseptic action of sulphurous acid, when introduced into the system, that sulphites administered by the mouth will, it is said, prevent decomposition and putrefaction of urine in the bladder." Here, then, we surely have an internal antiseptic of great and undoubted value, and which, under the circumstances, would certainly seem to merit a more extended and careful trial, as a prophylactic and curative remedy, than it has yet been recorded as having received in the human subject, in the prevention and rational treatment of septicæmia. (Dr. J. W. Burman, p. 175.)

ZYMOTIC DISEASES.—*Cause of Rise of Temperature in.*—It is by no means difficult to draw an analogy between the process of brewing and the process that goes on in the human body affected by zymotic disease. In zymotic disease there is (1) an organic fluid—the blood—susceptible of change; (2) this fluid may be fermented by the introduction of disease germs which act as ferments; (3) the fermentive process causes a rise of temperature; and (4) the kind of fermentation will depend on the kind of ferment or germ introduced, just as the quality of yeast will govern the sort of fermentation set up in the brewery; (5) the act of fermentation being accomplished, the mature form of the ferment disappears or perishes, but in so doing gives rise to a fresh crop of spores, which, *after an interval*, re-develope the mature form of ferment, and with it the fermentive process and a consequent further rise of temperature; (6) this sequence can be checked, more or less completely, by the application of cold, or by the administration of some bitter, such as quinine, which acts as an anti-ferment, just as the bitter of the hop acted in the beer. (Dr. John Shea, p. 41.)

AFFECTIONS OF THE NERVOUS SYSTEM.

COMA.—*The Thermometer as an Aid to Prognosis.*—After comparing the notes of five cases in which I have carefully recorded both temperature and pulse, I cannot but agree with Dr. MacEwen when he says in brain-lesions the temperature is far below normal; and in my opinion (though I do not for a moment wish to question such an authority as Dr. Richardson), given a case of sudden insensibility, *pure et simple*, in

proportion as the temperature is below normal so much the more is the probability of cerebral destruction evident, and in inverse ratio is the chance of the patient's recovery. (Mr. H. J. Forster, Lancet, Dec. 27, p. 942.)

NEURALGIA.—*Aconitia.*—Aconitia is perfectly successful in such forms of facial neuralgia as are not correlated with other lesions, which are not intermittent, and which have not a well marked recurrence; in other words, in those forms to which M. Gubler has applied the term congestive, and which are most frequently caused by exposure to cold. In such cases aconite produces a rapid cure within two or three days. Dr. Oulmont has even seen a case of facial neuralgia of seven days' standing, in which there was no well marked periodicity, and which had resisted sulphate of quinine, yield instantaneously and permanently to a quarter of a milligram of nitrate of aconite. The results are more marked and rapid in cases of recent neuralgia than in those of long standing. Examples are quoted, however, in which the affection had lasted for periods of one month, two months, and even five years, but which had yet been cured, the first on the seventh day, the second on the third, and the last in three weeks. Aconitia has also a distinct effect in secondary neuralgia, as, for example, in dental caries, otitis, paraplegia, &c. Acute rheumatic arthritis may be successfully treated with aconitia. On account of the energetic action of the remedy the susceptibility of the patient should be tested by administering, in the first place, three pills daily, each containing a fifth of a milligram of crystallised aconitia in addition to five centigrams of pure quinine; one in the morning, one at midday, and one in the evening. If no alleviation of the pain is experienced on the first day, the dose may be cautiously augmented by a pill per diem, until a maximum dose of six in the course of twenty-four hours is attained, and in the majority of cases it will not be necessary to overstep this limit. (Dr. Oulmont, p. 87.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ACCUMULATION OF CERUMEN SIMULATING CHRONIC BRONCHITIS.—Accumulation of cerumen in the ears and chronic bronchitis both chiefly affect those advanced in life. It may be that a connection between the two as cause and effect has hitherto been overlooked. However, in any case of prolonged and troublesome cough which resists the ordinary remedies, I should advise a careful examination to be made with a speculum of the external auditory canal, it being

almost needless to say that a considerable accumulation of wax may exist without damaging the hearing to any marked extent. (Mr. J. Rudd Leeson, p. 99.)

MUCOUS POLYPUS OF THE NOSE.—In some cases of polypus of the nose, I have recently been adopting a treatment which has given good results. In structure, these growths consist of but little more than connective tissue infiltrated with serum, and enclosed in something resembling mucous membrane; when removed by avulsion and exposed to the atmosphere, they rapidly shrivel by the escape of their serum; their distended grape-like appearance being exchanged in a short time for that represented by little more than a few shreds of connective tissue. The treatment to which I refer consists in freely puncturing these growths from the anterior nares by means of an ordinary acupuncture-needle, thus allowing the fluid of which they largely consist to drain away. To prevent them from refilling, I follow this up by ordering the patient to inject into the nostrils a solution of carbolic acid and glycerine, which has a most marked drying-up effect, and to continue to do this daily and thoroughly for some time. In this way, I have been able to deal successfully with some cases where the growths have been of a limited nature, and the patient averse to their avulsion. In the last case, I made the punctures with one of Southey's trocars, which answered well, the serum escaping through the canula. I have thus, in treatment, regarded these as being local and limited œdemas, rather than hypertrophies, and as being, when once emptied, curable by astringents. It is not always possible, from their position, to subject all these growths to puncture, otherwise I believe this plan would be found generally successful. (Mr. Reginald Harrison, British Medical Journal, Nov. 15, p. 773.)

NASAL CATARRH.—*Iodoform.*—I snuff up each nostril about as much iodoform as would lie on a threepenny-piece. I did this about six o'clock in the evening, and about eleven o'clock I was immensely relieved, so that the coryza was markedly diminished. In that short time, my cold had been transported from the cold stage to the stage of resolution. Next morning, I was almost well. I resolved to try this drug on various friends or patients, and have done so with great success. The coryza and accompanying pain and discomfort disappeared in a few hours, or in the course of a night. In cases of mild catarrh, I found it made the symptoms a great deal worse. I generally find one application enough, and give a smaller quantity than I used myself. (Mr. Albert Wilson, British Med. Journal, Jan. 31.)

NIGHT-SWEATS OF PHTHISIS.—*Picrotoxine.*—Quite recently, Dr. Murrell has introduced picrotoxine as a remedy for the night-sweating of phthisis. He uses a 1 in 240 solution in water, and of this he gives from one to four minims three times a day, the last dose being taken at bedtime, or immediately before the time at which the perspiration usually commences (p. 97).

PHARYNGITIS, AND HYPERTROPHY OF THE TONSILS.—In cases of pharyngitis, when the vessels are enlarged and tortuous and there is not much secretion, and in hypertrophy of the tonsils, the following solution should be painted on the parts twice a day:—Ergotin, gr. xx.; tincture of iodine, ʒ i.; glycerine to make ℥i. (Dr. W. A. Dabney, American Journal of Medical Science, July 1879.)

PHTHISIS AND BRONCHIAL AFFECTIONS.—*Petroleum.*—Dr. Milton, of Bradford, Pennsylvania, several months ago called the attention of the medical profession to the wonderful efficacy of crude petroleum as a remedy in consumption and bronchial affections generally. He has now prescribed the pills made from the dried crude petroleum in about fifty cases with the most satisfactory results. Bronchial and laryngeal troubles are relieved from the beginning of the first dose. In consumption, it has afforded more satisfaction than any other remedy that he has tried; in several cases of what might be termed incipient phthisis it has effected a permanent cure to all appearances. The crude petroleum is a popular domestic remedy in the oil country for most of the ills that flesh is heir to, but more particularly for coughs, colds, and bronchial troubles generally. The profession will be slow to adopt so simple a remedy as crude petroleum in consumption, but from the testimony now in Dr. Milton's possession from a great number of his medical friends outside the oil country, he has no hesitation in calling the attention of the profession to it as one of the very best means of cure in any curable case of phthisis pulmonaris. The only difficulty he has found has been the difficulty in getting patients to use the medicine in the shape of pills, and the nausea they frequently produce; but by perseverance this is easily overcome. The article which he usually prescribes is found incrusted on the bottom of the tanks, and has the consistency of putty, and is easily made into pills by incorporating it with any vegetable powder. The size of the pills is three to five grains; one pill from three to five times per day. The cough and soreness of the lungs are speedily relieved, the night sweats are increased. Out of the cases in which he used it, only three derived no particular benefit. They were

cases that had about exhausted the Materia Medica, and would not persevere. He has kept notes of twenty-five well-marked cases of incipient tuberculous consumption of the lungs; nineteen of which he considered cured. Three of these are now under treatment; the balance he has lost sight of. He has no confidence in it in advanced or confirmed consumption, where vomicæ have already formed. He thinks it acts by preventing inflammation in the lungs, thereby retarding any further destruction of the lung-tissue. It may act specially on the disease, but this is left for others to investigate. It has proved beneficial in cases of asthma. It has no effect on nasal catarrh, ozæna, &c. (Dr. M. Milton, Practitioner, April, p. 287.)

Diagnosis between Capillary Bronchitis and Phthisis.—There is not unfrequently some difficulty in forming this diagnosis. The thermometer is of much use. In capillary bronchitis, I have found that the temperature does not have the afternoon, or evening rise so common in phthisis; on the contrary, the temperature is highest in the morning. (Dr. A. T. H. Waters, p. 92.)

PULMONARY DISEASES.—*Climatic Influence.*—Dr. Schleisner says, "there are countries where phthisis is unknown, as for instance, Iceland," the very place where pneumonia is often rampant. In Finmark no phthisis is seen; and the Swedish physicians maintain that consumption becomes less common as we proceed northwards. Dr. Lombard of Geneva says he has never known a case of consumption among the monks of St. Bernard. In the cases of young children who are kept very close in heated rooms, and who are said to be always taking cold, we often see most obstinate cough and catarrh due to the throwing off from the air-passages of a weak poorly-nourished epithelium which in time may choke the air-cells, and so lead to pulmonary consumption. The cure consists in laying aside paregoric and squills while we feed the epithelium with a pure healthy air. Appetite soon returns, and the cough speedily takes to flight. In removing lingering inflammation after an acute attack on the chest, I have seen excellent results come from a sojourn at Torquay, Ventnor, and similar mild warm health-resorts; but when the disorder has passed from the inflammatory stage to one that involves the general nutrition, and that is marked clinically by softening and breaking down of lung-tissue, with night sweats and copious purulent expectoration, I never saw any good come of a residence in a mild sedative climate. (Dr. J. C. Thorowgood, p. 100.)

PUTRID EXPECTORATION.—*Local Treatment.*—The uselessness of internal remedies in these cases is well known. Treatment by inhalation, especially of the terebinthinates, has been tried, with a certain amount of success, more largely on the Continent than in this country, but the method adopted has been faulty. A few drops of the drug have been poured on the surface of hot water and inhaled for a few minutes two or three times a day, and a variety of cumbrous instruments have been suggested for this purpose. Two or three years ago, Dr. W. Roberts, of Manchester, described a simple portable "respirator inhaler," in the form of a metal box perforated in front and behind, and filled loosely with layers of tow on which the inhalation liquid was poured. This inhaler fits over the mouth, and is fixed by elastic bands over the ears like an ordinary respirator. The substances used for inhalation, and which are poured on a sponge in the front of the cavity of the respirator, are all well-known drugs—pure oil of turpentine, carbolic acid and thymol, either pure or diluted with from one to three parts alcohol, and creasote. Dr. Curschmann's application of them differs, however, from what most practitioners are accustomed to, in his using them either pure or, if diluted, but very slightly diluted. Both carbolic acid and thymol evaporate much more freely in alcoholic solution than when pure; and Dr. Curschmann has almost invariably used thymol in this form alone. Alcoholic solutions of carbolic acid are more apt to cause paroxysms of cough than the undiluted acid. More patients, however, object to the use of thymol than of carbolic acid; but the former is, no doubt, safer for children's use than the latter. Creasote never requires dilution, and on this point we are able to confirm Dr. Curschmann's experience, but it is very important to see that the druggist supplies a pure article. Curschmann prefers creasote in cases where there is a tendency to hæmoptysis: he finds that it not only has a styptic action and disinfecting properties as powerful as those of carbolic acid, but that its vapour is sedative and allays rather than excites cough. We have been surprised to find even physicians with a large *clientèle* of lung patients unaware even of the existence of so simple and valuable an instrument as Dr. W. Roberts' respirator inhaler. (Editor of Medical Times and Gazette, p. 89.)

SNEEZING.—*How to Cure Fits of.*—Mr. Bradley caught a severe cold, accompanied by annoying fits of sneezing upon the slightest impact of cold air. He says: I plugged my nostrils with cotton wool. The effect was instantaneous; I sneezed no more. Again and again, I tested the efficacy of

this simple remedy, always with the same result: however near I was to a sneeze, the introduction of the pledgets stopped it *sur le champ*. Nor was there any inconvenience from their presence, making them sufficiently firm not to tickle, and yet leaving them sufficiently loose to easily breathe through. This is really worth knowing; for incessant sneezing is among the greatest of smaller ills; and it seems only a rational conclusion to hope that, in this simple plan, we may have the most efficient remedy against one of the most distressing symptoms of hay-fever. (Mr. S. M. Bradley, British Medical Journal, Dec. 27, p. 1021.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

ASSISTANTS TO DIGESTION.—1. *With Malt Infusion.*—Three ounces (or three piled-up tablespoonfuls) of crushed malt are thoroughly well mixed in a suitable vessel with half a pint of cold water. The mixture is allowed to stand overnight—that is to say for twelve or fifteen hours. It is then filtered through paper until it comes through perfectly bright. The above quantities yield about seven ounces of product. Malt-infusion thus prepared has a light-brown colour like sherry, a faint sweetish taste, and the odour of beer-wort. The mode of proceeding with the malt-infusion is as follows:—A suitable gruel is prepared from wheat, or other flour, or from oatmeal, groats, pearl-barley, arrowroot, or any other farina. The gruel may be made with water alone, or, as is more usual, with the addition of milk or some kind of meat broth. In either case the gruel should be well boiled, and strained to separate the lumps. When the gruel or broth is cold, or at least sufficiently cool to be tolerated in the mouth, the malt-infusion is added. One tablespoonful (well mixed therewith) is sufficient to digest half a pint of gruel. The action is very rapid; in a few minutes the gruel becomes thin from the conversion of the starch. When this point is reached the food is ready for use. The only precaution to be observed in the process is to make sure that the gruel is at least sufficiently cool to be borne in the mouth before the malt-infusion is added. It is not of the least consequence if the temperature be below this point, for the transformation goes on just as well when the gruel is cold as when it is warm; whereas too high a temperature endangers the activity of the ferment, which is rendered inert at and above 157° F.

2. *With Extract of Pancreas* (*Liquor Pancreaticus*).—This possesses an extraordinary power as a diastatic agent. In this respect it far transcends any malt-extract. A sample,

made by infusing one part of fresh pancreas with seven parts of water, was found on trial, to possess fully twenty times the diastatic power of the above described standard malt-infusion. Pancreatic extract must therefore be considered as taking the first rank among the available artificial aids to the digestion of starch. It may be administered for that purpose exactly in the same way as the malt-infusion, but in reduced doses. (Dr. W. Roberts, p. 107.)

ARTIFICIAL DIGESTIVE JUICES.—*The Preparation of.*—The study of the digestive ferments has been immensely facilitated by a method first introduced by Eberle. Eberle discovered that an aqueous infusion or extract of the digestive glands possessed the same properties as the natural secretions or juices of those glands. The reason of this is that the glands which secrete the digestive juices contain within them a reserve stock of their respective ferments. Accordingly when the glands are infused in water their reserve stock of ferments passes into solution. These infusions or extracts then constitute artificial digestive juices which operate in a flask or beaker in the same way as the corresponding glandular secretions act in the alimentary canal. The following solutions are those best suited for the purpose:—

Boracic Solution.—This solution contains 3 or 4 per cent. of a mixture of two parts of boracic acid and one part of borax. An extract of the stomach or of the pancreas made with this solution keeps perfectly, and has little taste and no smell. For experimental purposes, this extract is, I believe, all that can be desired—it is neutral in reaction and is chemically inert. It answers well also for administration by the mouth when the dose does not exceed one or two teaspoonfuls. But when larger quantities are required for the preparation of artificially-digested food, and when food thus prepared has to be used day after day in quantity sufficient to sustain nutrition, larger quantities of boracic acid and borax are taken into the stomach than that organ can always comfortably tolerate.

Dilute Spirit.—The second solution is water mixed with 12 or 15 per cent. of rectified spirit. This solution makes a most excellent extracting medium, and the quantity of spirit in it is so small as rarely to be an objection to its use. In the preparation of artificially-digested food a final boiling is usually requisite, and in this final boiling the alcohol is dissipated. On the whole, this is perhaps the most generally useful of the three solutions.

Chloroform Water.—Chloroform dissolves in water in the proportion of about 1 in 200; these are the proportions em-

ployed in the preparation of the *Aqua Chloroformi* of the British Pharmacopœia. This forms a perfect solvent for the digestive ferments, and its keeping qualities are unrivalled. (Dr. W. Roberts, Lancet, April 3, p. 515.)

CONSTIPATION.—If we remember the power that the colon possesses of absorbing fluids (which numerous experiments place beyond a doubt), we do not require to ask an explanation as to the disappearance of the watery constituent of the stools in subjects suffering from constipation. When the fæces descend into the colon they are of a soft consistence; they are *not* hard and dry. What has become then of their watery constituents when they are converted into hard scybalous masses? Sometimes so difficult to pass are they, that an evacuation of the bowels in such circumstances may actually produce more suffering than the pains of labour. What becomes of the watery portion of the fæces, and what is the effect on the general system, especially the nervous apparatus, of the hardened matter lodging in the bowel? We have not to look far for an answer to the first part of the question when we are cognisant of the power of absorbing liquids which the large intestine possesses. The fluid most certainly finds its way into blood, and thus of necessity produces a form of blood-poisoning. We may note the following effects produced by the absorption of such fetid matter,—the red corpuscles become diseased, they are altered in colour, diminished in numbers, have their carrying power lessened. Thus a sallow complexion results, dark rings appear below the eyes where the skin is thin and more transparent, the limbs and extremities are cold in consequence of oxygen being supplied in less quantity. There is a feeling of lethargy due to the blood being vitiated and the corpuscles being in this enfeebled state. Consequently the system is not nourished, and there is a falling off in flesh. Then there is a diseased blood supply circulating through the nervous system, and as a consequence nervous depression co-exists. The pulse becomes slow and easily compressed. There is a feeling of numbness in the extremities, the organs of digestion and assimilation are lowered in tone. There is loss of memory, and a want of power of concentrating the thoughts, a heavy drowsiness supervenes, which never seems to be removed by what appears to be really sound sleep, and all the functions of the body are carried on in an unsatisfactory way. (Dr. Robert Bell, p. 121.)

DYSPEPSIA.—*Picrotoxine.*—Picrotoxine is recommended in various forms of dyspepsia, notably when there is severe epigastric pain aggravated by pressure or by taking food. Dr.

Phillips speaks of it as being of singular service when the colon is distended with flatus, and when the bowels are constipated and the motions hard and lumpy. (p. 97.)

DYSENTERY.—*Chronic.*—Abstain from active and special treatment, except to meet occasional and urgent symptoms. Instead of administering an occasional dose of castor oil, *systematise* its administration by giving it on stated days at least twice in the week, with the view not merely of removing retained scybalous fæcal matter and irritating discharges, but to prevent their accumulation by sweeping out the bowels at regular and stated intervals. (Dr. C. H. Ralfe, p. 117.)

Dysentery of Hot Climates.—Rectal Injections.—In the treatment of the dysentery of hot climates, the effort to soothe the mucous membrane of the rectum, and allay spasm of the sphincter ani, amply repays the attempt by tending to the production of the *sine quâ non* of rest for the diseased bowel and the patients generally. The attainment of this object is frequently much assisted by the action of bismuth used per rectum. Although I have given this remedy as an injection, simply suspended in mucilage in the case of children (or in adults where opium in some form was being administered by the mouth), my usual plan has been to combine forty grains with half a drachm of tincture of opium, and, if intolerance of ipecacuanha by the mouth was encountered, also adding two scruples of that drug. The soothing influence of a bismuth and opium injection is beneficial after the employment of injections of nitrate of silver. In all cases, however, treatment by this method has been undertaken, as with Dr. Houghton, as merely subsidiary to the use of ipecacuanha by the mouth, if practicable. In the treatment of endless cases of famine diarrhœa and dysentery encountered during the late wretched famine in Southern India, the use of bismuth in ten-grain or scruple doses, combined with compound ipecacuanha powder, proved, after an honest trial of every feasible method, to be the most reliable treatment. Given at a time when it is calculated the intestine is fairly quit of food substances, this combination at once soothes the frail atrophied mucous membrane, and affords a film which protects its surface from the action of changed and irritating secretions. (Dr. W. G. King, p. 119.)

HEMORRHOIDS.—*The Clamp and Cautery.*—The operation consists in drawing down and embracing the tumour within the blades of the clamp; then removing the hemorrhoid with a sharp pair of scissors, and, lastly, applying the actual

cautery freely to the cut surface. It is very important to isolate the tumours well, so as to compress them easily, and in some cases where the hemorrhoid runs, as it were, abruptly into the surrounding hypertrophied skin, it is well just to make a slight groove with a pair of scissors. By this means the blades of the clamp compress the growth more readily at its neck. In large tumours I use a clamp furnished with a light but powerful screw near its handles. By this means I tighten the grasp of the blades of the clamp more firmly; then, in removing the part, I take great pains not to divide the base of the tumour too close to the clamp. I leave tissue to the extent of an eighth of an inch, so as to have a fair surface for the use of the cautery. The iron is applied very carefully and very freely at a black heat, and then, all the cut surfaces being so treated, the blades are gently and gradually released by the screw on the clamp. In this way the pressure is taken off the neck of the tumour, and if any vessel is not thoroughly cauterised it immediately bleeds. The blades are then quickly screwed up again, and the hot iron is applied to the vessel until it ceases bleeding. Sometimes it is necessary, as in a case of huge prolapsus, to reapply the hot iron several times to the same vessel before bleeding ceases. You may, however, rest assured that with proper care you can stop bleeding from any vessel here by the use of the cautery. Each tumour, and there are generally three or four, is treated in this way, and then, when an opiate suppository is introduced by the finger well oiled, the operation is completed. The after-treatment consists in locking up the bowels by occasional opiates, and strictly dieting the patient. (Mr. Henry Smith, p. 211.)

HEPATIC AND INTESTINAL STIMULANTS.—*Podophyllin.*—With somewhat small doses the increased bile-secretion is much more prolonged, although the hepatic excitement is not intense at the outset.

Iridin and Euonymin.—These two substances, although not so powerful as podophyllin, will both doubtless be preferred in many cases to that substance, because of their far milder excitement of the intestine. For not only is the latter in most cases advantageous on its own account, but also because the action on the liver is far less liable to be hampered and diminished by the intestinal stimulation. This is apt to be the case with podophyllin. The average dose of iridin is four grains; of euonymin two grains. In either case two grains of extract of hyoscyamus should be added, and taken at bedtime, for without this some persons expe-

rience griping. Neither substance produces headache or any sickness. In some persons the above doses of both substances produce a sufficient purgative effect; but in other cases the purgation is insufficient or delayed, and griping is then apt to ensue. The most beneficial result is obtained by following the dose of these remedies by a mild saline aperient, such as Püllna or Carlsbad water, on the following morning, so that the bile secreted during the night may be fully and quickly removed. I have in my own case noticed slight depression after four grains of iridin, which I never observed after two grains of euonymin. I therefore inferred that the latter is preferable when repeated stimulation of the liver is desirable. It is, however, important to remember that although euonymin usually suffices to quickly remove a slight feeling of biliousness; iridin is, I am convinced, the more powerful remedy of the two, when the tongue is decidedly yellow. I have in such a case been more than once surprised to find that on awaking in the morning after taking four grains of iridin the previous night the yellow tongue and the bilious sensations were entirely gone. Since the publication of our results these remedies have come into very general use. Mr. Hardyman, of Cardiff, states that he has used euonymin in two-grain doses at bedtime in over fifty cases of biliary derangements and sick headache, and finds it of much value. Finding that in most cases one dose is insufficient he gives two grains at bedtime on two successive nights, following it each morning with a saline purge. (Dr. W. Rutherford, p. 132.)

Intestinal Obstruction.—*Administration of Enemata.*—In administering enemata in intestinal obstruction it is not sufficient to give one injection, and, as soon as that returns, to desist. The method I adopt is to fill the intestines, as far as possible, with the fluid used as an injection again and again, even to half a dozen times if necessary, until a satisfactory action of the bowels has been induced. In giving enemata in this manner, only warm water and soap are necessary, as turpentine, &c., are likely to cause action of the bowels before sufficient time for the softening of the fæces has elapsed. Care should be taken also that the water is not too warm, or it will be speedily expelled from the bowels. I generally commence with the water 96°, and increase the temperature gradually in the later injections to 105° or 106°. The enema should be injected slowly and without the use of force. (Mr. G. Brown, p. 111.)

AFFECTIONS OF THE URINARY SYSTEM.

CALCULI.—*Vesical Calculi.*—It is of the utmost importance that the presence of a calculus in the bladder should be ascertained before it has reached any considerable size; and the bladder should always be explored with a sound on the occurrence of the earliest symptom. All small and moderate-sized calculi may be removed by the lithotrite, and, as a consequence, when this doctrine is accepted and carried into practice, lithotomy will gradually be abandoned, except in cases of an exceptional nature as regards the stone and patient. The importance of searching for stone on the occurrence of *one* of the first four symptoms alluded to—that is, before there are any symptoms of cystitis, which is almost always accompanied by alkalinity of the urine—is rendered evident by a knowledge of the intimate structure of calculi; for an examination of any considerable collection of calculi shows that in the vast majority of examples, the nucleus of a calculus is uric acid or oxalate of lime, and that a phosphatic calculus is a very rare thing. When such a phosphatic stone does exist, it consists in almost every instance of an aggregation of smaller pieces held together by inspissated mucus, or which have become adherent to one another first by mutual pressure and then by consolidation of the intervening substance. Such calculi are somewhat porous, like pumice-stone. Now, as oxalate and urate calculi are hard and presumably of slow growth compared with phosphatic, it follows that in the history of all calculi or nearly all, there is a time, and a considerable time, when they are small, hard, and of slow growth, and that is just the time when their presence may be indicated by the symptoms—pain, desire to micturate, blood, interference with the flow—occurring either singly or at considerable intervals, with intervening periods of total cessation of these symptoms, so as to throw the patient and even the medical adviser off their guard. And this is true to some degree even of the small number of calculi which are phosphatic from their first origin. I think, then, we may conclude that if only the patient would apply on the first occurrence of anything unusual connected with his urine or the region of the bladder, and the medical attendant would have the bladder at once explored, calculi would be detected when of very small size, in almost all instances. (Dr. G. Buchanan, p. 226.)

Renal Calculi.—It may be taken as a broad rule, to which, however, of course, there may be exceptions, that when a calculus originates in the kidney the nucleus is either of lithic-acid or oxalic-acid formation; whereas, on the other

hand, if the stone is formed in the first instance in the bladder, the nucleus is either phosphatic or consists of some foreign body in the bladder, probably introduced into that viscus from without. All renal calculi may be said, therefore, to be constitutional and to arise from some morbid state of the urine, this state or condition depending on malassimilation. (Mr. T. P. Pick, p. 217.)

CATARRH OF THE BLADDER.—*Albumen in the Urine.*—A marked distinction exists between the significance which albumen possesses in two different categories of cases. 1. When a patient's urine, habitually clear, acid, and free from the faintest blood-tint, throws down to the test of heat and nitric acid, a notable quantity of albumen, the source of that albumen is the renal circulation, and, if persistent, the case is almost certainly one of grave import. The presence of organic change in the kidney structure is to be inferred, and other evidence of its existence, if sought for, will probably be found. 2. A very slight admixture of blood in any urine, no matter what the source of the hemorrhage, will produce a considerable deposit of albumen. It is evident, then, that the product in such cases, although sometimes grave, is not necessarily so, and that it may furnish an indication of the slightest possible import, inasmuch as a little blood may appear in the anterior passages, from a lesion which is slight and temporary in its nature. 3. Pus in the urine may, and most commonly does, proceed from some local condition of the bladder, occasionally, indeed, from local inflammation of the urethra. Nevertheless, albumen will be deposited on applying appropriate tests. It is evident that albumen, resulting simply from pus produced by chronic cystitis, has an import vastly less grave than that described above as No. 1, being a purely local and mostly temporary affection of an organ which has no vital function, but merely a mechanical one; the albumen in the former instance being evidence of disorganisation in the structure of a vital organ —that is, one the sufficient action of which is essential to the very existence of the body. The important practical point in relation to treatment is first to ascertain the occasion of the local catarrh. In nine out of ten of these cases it consists in inability, often only to a slight extent, on the part of the patient to empty the bladder completely. The universally acknowledged cause, hypertrophy of the prostate, is, of course, the first in order of frequency. But after this are others not infrequent. Defective action may be due, first, to simple atony, the result of past habitual or occasional overdistension of the bladder with urine; secondly, to thickened

and incompetent muscular parietes of the bladder after chronic inflammation, sometimes associated with old stricture; thirdly, to defective innervation seen in connection with other slight signs of impaired function in a nervous centre; the last being, of course, the most serious of all, in its nature and probable results. In all of these local treatment, by carefully removing all the secretion by means of a soft catheter two or three times a day, perhaps aided by gently washing out some remainder, is the chief efficient remedy. Remember that this incompetence of the bladder is always to be sought for by physical examination; no other form of evidence in relation to it, as the patient's sensations, &c., is to be accepted as trustworthy. The introduction of a soft catheter immediately after the patient has passed water by his natural efforts, is the only test, and it should be applied on two or three occasions before arriving at a definite conclusion. (Sir H. Thompson, p. 215.)

DIABETES.—*Alkalies.*—Dr. Cornillon has studied at Vichy the effects of the waters upon diabetic patients. He finds that after the fourth or fifth day, sometimes even sooner, the thirst and dryness of the mouth become less troublesome, and inappreciable after the tenth day in favourable cases, and after the thirtieth day in the most obstinate. The patient passes better nights, and the sleep is calm. During this period the urine, previously acid and light coloured, has become alkaline and of a yellowish orange. (p. 138.)

EXAMINATION OF THE INTERIOR OF THE BLADDER.—*The "Nietze-Leitner" Endoscope.*—By means of this instrument the interior of the bladder can be illuminated and examined. It is certainly a daring achievement to carry a platinum wire heated to whiteness into the bladder, and to use it there for some minutes in the act of research; and it is a marvellous success as regards the mechanical arrangement by which this result is effected with safety. The idea originated with Dr. Nietze, now of Vienna, but its realisation is due to the patience and perseverance of Leitner, the surgical instrument maker of that city. There are some mordid conditions the existence of which we sometimes suspect, but cannot positively affirm to exist, whose presence may now be ascertained through the agency of the new endoscope. I refer to the identification of sacculated stone as the cause of persisting and unrelieved symptoms; to the detection of pedunculated growths and of villous disease of the bladder, removable by operation; and, lastly, to the investigation of the nature of foreign bodies, other than calculi, which have become lodged there. I have lately seen a fatal case of vesical growth which might have

been easily removed by operation; and in such a case, as well as in the rare contingency of a foreign body, the new endoscope may possibly render essential service. A precise knowledge of the nature, size, and position of a foreign body might enable us to devise a safe and certain means of removing it, in place of a tentative, uncertain, and hazardous proceeding. (Sir Henry Thompson, Lancet, Dec. 6, p. 823.)

GONORRHŒA, AND GRANULAR URETHRITIS.—Dr. Eldridge gives details of some cases of gonorrhœa and granular urethritis which he has treated by ergotin locally, with marked success. The remedy may be introduced into the urethra either by means of the ointment syringe or rubbed into the meshes of a cylindrical hollow lamp-wick, which is supported by a small bougie passed into its centre, this swab being allowed to remain in the urethra for about half an hour. (Dr. S. Eldridge, New York Medical Journal, Oct. 1879.)

LATENT ALBUMINURIA.—As a general rule, it will be found that, whatever may be the cause of the albuminuria, the albumen is twice as abundant after food and exercise as before breakfast; and I have met with a considerable number of cases in which the urine, before breakfast having been free from albumen, is found to be more or less copiously albuminous after food has been taken. In all cases, therefore, of actual or suspected albuminuria, I ask to be supplied with two specimens of urine: one passed on rising in the morning, the other secreted two or three hours after a meal. In some exceptional cases, food has less influence than exercise on the production of albuminuria. A distinguished London physician, whose successful career was cut short by degeneration of the kidneys, found that, at the onset of his malady, albumen appeared only occasionally after walking exercise, when it was present in large amount. Food at the commencement appeared to have no influence on the production of albuminuria; but after a time there was persistent albuminuria, and ultimately death from uræmia. (Dr. George Johnson, p. 136.)

LITHOTOMY.—*A Method of Performing.*—The proposal which I make is: that, the membranous urethra being opened behind the triangular ligament, the deep incision—the incision in the prostate gland and the neck of the bladder—shall be made upwards and to an extent suited to the size of the stone. The extent, I believe, need rarely reach the capsule of the prostate. Where the firm fibrous submucous tissue is divided, and the substance of the prostate scored, the dilatability of the parts is singularly great. Touching the details: the first incision may be made as in the median or in the lateral

method. The latter seems to me to give easier access to the apex of the prostate. I put a little angle in the outer wound, so that the concavity is towards the rectum, and all parts of it are more equally distant from the rectum. The membranous urethra should be freely opened. The well-oiled finger should then be gently carried along the staff through the uncut prostatic urethra, and brought into contact with the stone. The staff having been taken out, the finger firmly depresses the neck of the bladder downwards and backwards, while a straight blunt-ended bistoury is passed flatwise along its dorsum until the bladder is reached. The instrument, having its edge turned upwards, is carried by a quick movement a short distance into the bladder, and, with the edge still upwards, withdrawn in contact with the dorsum of the depressed finger. This proceeding gives a fairly uniform diameter to the wound. As a rule, pressure of the knife upwards is not needed; the urethra, being stretched on the finger, is incised to a sufficient degree by a single sawing movement. If the stone be known to be very large, free incision upwards is easily made, and in a direction which readily allows a large stone to be removed. The forceps is next slid along the surface of the still retained and still depressed finger, and the operation is concluded in the usual way, save that all manipulation by finger or instrument is directed downwards to the floor of the neck of the bladder and to the prostatic urethra. The staff in this operation should not be large (especially if the operator's finger be large). A staff large in the membranous urethra, and prolonged probe-like into the bladder, might be partially withdrawn while the finger was insinuated into the bladder; or a probe might be run into the bladder, as in the median operation, prior to the withdrawal of the staff. A very simple expedient may be adopted where the urethra is small, which is especially useful in children. It is an expedient which some surgeons may prefer in all cases. A concealed bistoury, larger and stronger than the one used for dividing strictures at the meatus, may be easily carried into the bladder, after the membranous urethra is freely opened, along the staff (or even without the staff) in the urethra. A nervous operator may touch the stone with the instrument; the blade is then directed upwards and projected to a measured extent, and the instrument is withdrawn with the sheath pressing back towards the rectum. It seems to me that no lithotomy could be more simply contrived or formulated than this: open the membranous urethra on a grooved staff; put a strong simple concealed bistoury into the bladder; project the blade upwards and draw out the bistoury; put the finger into the

bladder, feel the stone, and extract as usual. Much may be done with this bistoury. It may be used as a sound; it may incise the prostatic urethra to any desired extent. After the incision (or before, if the urethra be large enough), the sheathed bistoury flatwise is an excellent guide for the finger to the bladder. All these advantages I experienced in the case of this little boy—my fourth case. (Mr. F. Jordan, p. 222.)

LITHOTRITY.—*Bigelow's Operation.*—As far as now thirty-one consecutive cases thus managed enable me to determine, I think the principle of Bigelow a decided step of progress for lithotrity. Let the stone as to size be within the limit of the surgeon's powers, and let all be removed, or at all events the greater part of it, at the first trial. In attempting this, he should commence by crushing freely with a light but strong lithotrite, removing *débris* by the aspirator-current afterwards. This done, the lithotrite is again introduced, if necessary, to deal with the remainder, and is again followed by the aspirator. If the rattle of fragments in the bladder against the end of the evacuating catheter still exists after its use, the lithotrite is to be again introduced to break them down, and another employment of the aspirator will perhaps complete the task. If only a few small pieces still remain, it is better perhaps to leave them than to subject the parts to repeated manipulations in order to remove some tiny portion. Such will do no harm, and may be removed at a second sitting. By this method, instead of limiting the sitting, as at one period was my practice, to a term of two or three minutes, it may extend from five or seven to twenty-five or thirty minutes, although a watchful caution must control the period as it becomes extended to the longer term named. For the purpose of this mode of operating, I have modified my instruments somewhat. I have used one lithotrite when necessary slightly stronger than before, making the male blade more wedge-shaped, or rather like the prow of a ship, in part angular, in part more bluff or obtuse; this male blade protrudes through a small opening at the bottom of the female blade, so as to make blocking up impossible. And the original aspirator of Clover, the principle of which is maintained in its entirety, I have considerably modified in a manner so that my new form of it offers three advantageous conditions: first, there is no possibility of air entering it; secondly, it ensures the shortest possible route into it from the bladder; and thirdly, it has a globular trap at the bottom, into which the fragments enter, but which they cannot leave. (Sir H. Thompson, p. 235.)

In the performance of lithotrity instrumental injury was

more dreaded than any harm that might arise from the irritation of fragments of stone within the bladder, and the removal of a moderate-sized stone from the bladder by lithotrity was a process prolonged generally over some days, and not rarely running into weeks. Prof. Bigelow's operation is founded on different principles, and necessitates an entire change in practice. He maintains that the bladder is comparatively tolerant of the presence in its interior of smooth polished instruments of a large size, even for a considerable time, but exceedingly intolerant of the rough and angular fragments of a broken stone. Thus the object of Prof. Bigelow is, if possible, to crush and remove stones at one sitting, believing that the advantage of complete evacuation of fragments is cheaply purchased by an operation prolonged, it may be, for an hour or more. His operation, then, is planned for rapid and complete crushing and immediate evacuation of the fragments, and it has already been applied successfully to stones of far larger dimensions than a prudent lithotritist would have ventured to attack by the old operation. He employs a strong scoop-lithotrite, with a very wide female blade, so constructed that it can scarcely entangle the mucous membrane of the bladder within its blades if used with ordinary care, and having in the female blade an opening just beneath the spur of the male blade, which prevents the instrument from becoming clogged with the *débris* of the stone, and admits of a thorough pulverisation of the stone without withdrawal of the lithotrite. For the rapid removal of fragments, Bigelow uses a series of large-sized catheters, running up to No. 20 English scale. These are straight, having an eye close to the extremity, well rounded off at its margins, and being exactly the size of the lumen of the catheter. The catheter is connected with an exhaust bottle by means of india-rubber tubing, and is removable at a bayonet-joint. The india-rubber exhaust bottle is much stronger than Clover's, and has a glass trap for fragments attached to it below, also fitted with a bayonet-joint. The bottle is worked in the same manner as Clover's; being compressed it distends the bladder with water, and on the removal of pressure its elastic recoil sucks out the fragments of stone. We are not yet in a position to judge accurately of the value of Bigelow's operation as a curative measure; the materials for forming a sound conclusion do not yet exist, at least in a form available for comparison. If Bigelow's practice be founded on truer views of the whole subject (as it seems likely that it is), the domain of lithotrity has been greatly enlarged and a real advance will have been made in the treatment of stone in the bladder. (Mr. T. Smith, p. 230.)

STRICTURE OF THE URETHRA.—*Gradual Dilatation.*—Treatment by gradual dilatation with instruments, allows the patient to follow his occupation. There is a general consensus of opinion amongst surgeons all over the world that this is the best method of treating ordinary dilatable uncomplicated stricture. It is attended with no risk to life, and involves no sacrifice on the part of the patient. Certain American surgeons object to it, alleging that the treatment is merely palliative. Inasmuch, however, as I consider no more can be claimed for any other method, I hold that their objection is not founded on good grounds. With what kind of instruments ought the process to be carried out? We have soft and metal bougies, and each have their respective advocates, although I am glad to say the partisans of the cold steel are gradually diminishing in numbers every year, to the infinite benefit of mankind at large. If anyone has a doubt as to which is the better instrument to use, a soft or a metal one, let him one day pass a soft one on himself, and a few days afterwards introduce a metal one. We need not wait for an answer. (Mr. W. F. Teevan, p. 247.)

Gradual dilatation by the passage of bougies of increasing sizes at short intervals is the safest of all methods, as it excites the least irritation when being carried out; but, unfortunately, it often fails to obtain an expansion which does not speedily shrink again; and this is especially the case for strictures of the antescrotal portion. It is most efficacious for strictures of recent formation—cases that consist mainly of inflammatory thickening, and especially if these be situate at the bulbous or bulbo-membranous portion. Again, gradual dilatation by the interrupted passage of bougies is sometimes the only prudent course when the patient has evidence of renal disease. Such persons run some risk of acute suppurative nephritis, and even of septic affections, if submitted to cutting operations. But, with these general exceptions, gradual dilatation is an uncertain and most unsatisfactory mode of treatment. Nevertheless, from its simplicity, safety, and non-interference with the patient's pursuits, bougie-passing is very generally applied to all strictures. The following are the chief characteristics of rigid and flexible instruments when compared with each other. Rigid instruments are far more difficult to pass, more capable of injuring the patient, and, unless passed with proper skill, more painful to him. On the other hand, they are more durable, can be easily kept in good condition in hot climates, can be made more smooth and, being rigid, their points can be guided by the hand past pouches and other obstructions. Of flexible instruments, the great advantage is their supple-

ness. This enables them to accommodate themselves to the urethra without strain; but, as their points cannot be easily directed away from an obstruction or false passage, they often slip where a rigid sound readily gains the bladder.

To pass a Sound.—Pass your sound thus:—the patient lying flat, shoulders low, especially if he be corpulent, so that his belly may be out of the way of the stem of the sound,—go to his left side and hold the sound, beak downwards, lightly by the handle between the thumb and first two fingers. So held, the impediments experienced by the point of the sound are most quickly detected; and, which is equally important, you cannot inadvertently give the instrument a thrusting impetus; for it must always be borne in mind that no more force is to be used with a rigid than can be applied to a flexible instrument. Then take hold of the penis behind the glans between the middle and ring fingers of the left hand, leaving the thumb and forefinger to push back the foreskin and expose the meatus. Next, holding the instrument across the patient's left groin, draw the penis on to it with the left hand; the right hand, after it has for a moment directed the point against the floor of the urethra, remaining motionless. When four inches of the sound have disappeared, carry the right towards the umbilicus without raising the sound from the patient's body; then push onwards the beak while the stem is raised from a nearly horizontal to a vertical position, and from that lowered again towards the patient's feet. By a proper combination of these two movements—the onward passage and the elevation and depression of the stem—the beak is kept sliding against the roof of the urethra, slips through the perineal fascia, and enters the bladder without catching in any of the irregularities. In carrying the instrument through the curve, no force must be used; nor must the rapidity of this sweeping movement exceed the rate of progress of the beak, or that point will be tilted upwards too much to allow it to pass under the symphysis pubis, and a violent strain will be put on the urethra by the long leverage of the stem, besides causing an abrupt arrest of the progress of the sound. (Mr. Berkeley Hill.)

To pass Flexible Bougies.—Commence as for passing the rigid sound, and, pushing back the prepuce with the left thumb and forefinger, draw gently forwards the whole penis, so as to stretch the spongy portion of the urethra. Introducing the bougie with the right hand, holding it lightly with the tips of the fingers, direct its point at first along the floor to avoid the navicular fossa in the roof. After that, simply push the instrument steadily onwards. If it catch at any

point, usually the floor of the bulb, use no force to push it on—that only deepens the depression in which the bougie has caught; but withdraw it a little, and roll it between the finger and thumb to disengage the point and make it, in its somewhat twisted condition, press against another side of the urethra. Should it still hitch at the same place, after two or three patient attempts, withdraw the bougie and employ one with a different point—an English one in lieu of a French one, or *vice versâ;* so that the impediment which arrests one may not be struck in the same way by a point of another shape. Give the English bougie a little curve by softening it well in hot water, and then plunging it into cold water while holding the instrument in curve. Such a point can be directed along the roof of the urethra, and thus escape the pitfalls of the floor. (Mr. Berkeley Hill.)

Gradual Dilatation by Continuous Tying-in.—This consists of keeping a flexible catheter, or filiform bougie if the narrowness of the stricture require it, day and night in the stricture; replacing it every forty-eight hours by a larger one, until the natural calibre of the urethra is restored to the strictured part. This replacement is needed, lest, being too loose in the urethra, the catheter slip out of the stricture, and so interrupt the process of dilatation. But the enlarging process is kept up quite as rapidly by a small catheter as by one which nearly fits the stricture. This process usually occupies from eight to twelve days. The method is easy, rapid, and safe. It requires three precautions. 1. The catheter should always be a loose fit; that is, two or three numbers smaller than the stricture. 2. The patient should be confined to his room, not necessarily in bed, but to avoid walking about, though I have known adventurous spirits who have gone to and from the City daily during the process. 3. The catheter should not quite reach the bladder, but its beak should lie in the membranous part of the urethra, leaving the neck of the bladder free. When it is desired to draw off urine, the catheter is pushed into the bladder while the urine flows, to be again withdrawn and the end stopped by a little spigot when the bladder has been emptied. Flexible instruments are tied in place in several ways. The plan I adopt is to pass the catheter through the stricture till it has reached the bladder and urine escapes. I then withdraw the catheter till urine ceases to run, and tie tightly round the catheter, an inch or so beyond the meatus, a piece of silk or cotton twist, of which the ends are again knotted together about one inch from the catheter, then carried round the penis behind the glans and tied. The foreskin is then drawn forwards, the

catheter shortened by cutting off the superfluous three or four inches, and the spigot put in. (Mr. Berkeley Hill, British Medical Journal, Nov. 22 and 29, pp. 804, 853.)

Internal Urethrotomy. — If the stricture be very tight or crooked, a slender olivary bougie is wriggled through it into the bladder. A staff, filled with a stylet, is then screwed on to the bougie and made to follow it: the latter coiling up in the bladder. The stylet is now withdrawn, when urine will escape by flowing out of the open slot where it emerges from the penis. The knife, covered by the sheath, is then placed inside the slot, the latter being outside the staff. The sheath not only protects the healthy urethra, but it also represents the finger in the urethra, and tells the surgeon where to begin to cut and where to leave off. The urethrotome, as modified by me, meets, I believe, all requirements. At a period not very remote the principle of dividing strictures from behind forwards was the one in vogue at the special department for urinary diseases at the Necker Hospital, at University College Hospital, and at St. Peter's Hospital. Now all is changed, and at each of those institutions strictures are incised from before backwards. (Mr. W. F. Teevan, p. 251.)

The patient is first anæsthetised; and, if the natural gauge of the urethra have not been previously ascertained, the passage is explored by a bullet-sound, or, if there be stricture near the meatus, by the urethrometer; the largest bullet-sound or widest expansion of the meter that slides without force along the urethra in its non-strictured part being taken as the size to which the strictured part must be incised. Should the meatus be too contracted to permit the wedge to enter, that is divided by a touch of a bistoury. The meatus being freed, a split sound is next passed to the bladder. The two halves of this sound are welded together at the point, but separated for the rest of their length. When closed, they equal in size No. 2 of the English and No. 8 of the French scale. When the stricture is tight or tortuous, the introduction of the split sound is facilitated by previously passing a filiform guide-bougie (No. 2 French) of silkworm gut mounted with a screw-head, to which the split sound may be screwed. Should it be doubtful whether the guide has reached the bladder, a No. 3 French (No. $\frac{1}{2}$ English) flexible catheter is screwed to the guide-bougie, and the whole pushed onwards till the catheter has passed eight inches inwards. A small exhausting syringe is then applied to the catheter, and a few drops of urine are drawn through it. The communication with the bladder being put beyond

doubt, the catheter is withdrawn and unscrewed from the guide-bougie; the split sound is screwed on instead, and then guided into the bladder. In practice, this preliminary testing of the path followed by the guide-bougie is seldom needed; the presence of the end of the bougie in the bladder is sufficiently indicated by the freedom with which it can be pushed in and withdrawn, having no tendency to slip out spontaneously, as has the bougie which is engaged in a false passage or is only partly through a stricture. The split sound being thus safely in the bladder, a wedge containing a concealed knife is then pushed down the urethra between the halves of the split sound, on which it runs in dove-tail grooves. The wedge separates the blades of the split sound, and when a stricture is approached sets tight the constricting fibres. The knife is then pushed forward between the separated blades, and, dividing the tense fibres, allows the wedge to advance until it traverses the urethra from end to end. The knife retreats within the wedge, and is only protruded when a tight band opposes the free passage of the wedge. Thus, band after band of fibres are cut, and a calibre is given to the urethra, equal to the size of the easily fitting bullet-sound previously used. The size of the wedge employed depends upon that of the urethra, and may be obtained either by having a wedge that expands like a parallel ruler, and so can be set to any size; or, by having a series of wedges, Nos. 24, 26, 28, 30 (French), being found sufficient in practice to meet the varying widths of different urethræ. Of course, one split sound and one knife fit them all. After the strictures are fairly divided, the wedge-cutter is removed, and a wedge a size or two larger may be passed without force along the split sound to test the completeness of the division of the strictures. After the split-sound is removed, a flexible catheter of moderate size (No. 20, French) is passed, and tied in for twenty-four or forty-eight hours; that is, until free secretion of mucus allows it to be withdrawn easily. (Mr. Berkeley Hill, British Medical Journal, Dec. 6, p. 883.)

External Urethrotomy.—External urethrotomy with a guide, an operation perfected by Syme, is with us reserved for cases where the perineum is honey-combed by abscesses and urinary infiltration, with considerable amount of subpubic induration of the urethra. Here, external section is preferable, as it gives free drainage through the wound, and thus obtains speedy closure and sound healing of the fistula. Syme's operation requires a curved staff, of which the five inches next the handle are straight, and as large as 20 or 22

French; beyond this, the staff is narrowed abruptly, to the size of No. 7 French, and grooved on its convexity in the mesial line from the shoulder or thick part to the tip. The operation begins by passing this staff along the urethra to the bladder, until the narrow part has traversed the stricture, and the thick part is arrested by the contraction. The urethra is then opened on the grooved staff, held steady by an assistant, so that its curve projects well in the perineum, and the knife at last reaches the groove. The edge of the knife is now directed forwards, and thus the bands of stricture which check the onward passage of the thick part of the staff are thoroughly divided, till no obstacle remains. The staff is then withdrawn, and replaced by a catheter. Here often begins a difficulty; the part of the urethra from the wound to the bladder cannot be hit by the beak of the catheter. This obstruction is prevented, if the tapering gorget of Mr. Wheelhouse be led along the groove of the staff to the bladder before the latter is removed. The gorget then acts as a guide for the catheter past the wound to the bladder. (Mr. Berkeley Hill, British Medical Journal, Dec. 6, p. 886.)

If retention from impassable stricture should set in, we then have a case no doubt the most difficult in all surgery, but it has been greatly simplified through the labours of Avery, Wheelhouse, Gouley, and myself. The operation, as performed by Wheelhouse, consisted in opening the urethra half an inch above the stricture, and insinuating a probe through it into the bladder. Gouley substituted an olive-tipped bougie for the probe; and as the stricture is often very tortuous it is clear the flexible bougie may pass where the inflexible metal probe cannot. Gouley uses the bougie as a guide to cut along, and slips his tunnelled catheter over it into the bladder. The great defect in the operation, as performed by Wheelhouse and Gouley, is that the position of the probe or bougie is not proved before cutting; hence either may have gone into a false passage instead of the true channel, without the operator being made aware of his mistake. Now I prove the position of the bougie by sliding over it a fine metal catheter, open at both ends, and having a slit in its convexity, along which the tenotome can be pushed. If the bougie be in the bladder urine will escape from the catheter when the former is withdrawn. I then divide the stricture *subcutaneously*, and to ascertain that this is completely effected I slide over the silver tube, a large elastic one, four inches long, which will slip into the bladder if the road is clear. (Mr. W. F. Teevan, p. 255.)

AMPUTATIONS, FRACTURES, AND DISEASES OF THE BONES, JOINTS, ETC.

ABSORBENT AND ANTISEPTIC SURGICAL DRESSINGS.—First as to the action of the materials employed. Here is a piece of the finest cotton wool, such as is ordinarily used by jewellers. It floats on water, and rises to the surface any number of times after being pushed to the bottom of the glass. It will float for weeks. Drop into a tumbler of water this pledget of cotton wool, made absorbent by the removal of oily matter and other impurities, and you see that it sinks to the bottom in a very few seconds. Envelope a pledget of absorbent cotton in a piece of unbleached gauze, such as is commonly used in surgical practice. In spite of the proved absorbing power of the contained cotton, so impermeable is the gauze that the pledget floats and resists forcible immersion. If, however, you make a little pad with the absorbent cotton and the same kind of gauze, bleached, you will find the substance sucks up the water with avidity, and sinks at once. So with bandages. Here is a piece of ordinary calico bandage; it floats in the basin like a plank on a pond. Here is a piece of open wove bandage absorbent; it sinks the instant I drop it on to the water. Once I had ascertained these facts, I lost no time in putting them to the test of clinical experience, which has demonstrated the great therapeutic value of the absorbent materials; discharges drain through them so readily that wounds are kept clean and the surrounding parts dry. Union rapidly and painlessly consolidates under the elastic pressure. So great is the elasticity of the material that the pad I hold in my hand is scarcely flattened, though it has been firmly bandaged on a man's instep for five days. On holding the pad before the fire you see it puff up at once, and quickly regain its original fulness and downy softness. Here is another pad, soft and elastic, though for a week it has been firmly bandaged to a man's foot, inside a millboard moulded splint. Here is one of the absorbent antiseptic pads which Messrs. Southall Brothers and Barclay have prepared for me, and which is beautifully soft, though it has been tightly screwed down in my copying press for upwards of nine hours. The absorbent materials lose none of their physical properties when treated with borax, iodine, tannin, and similar substances. Those researches have been continued, and largely extended with a view to practical application, by Messrs. Southall Brothers and Barclay. Besides the substances named, benzoic and carbolic acid have been employed, and before you are gauze and cotton wool tissue, pads and bandages, perfectly absorbent and elastic, and powerfully styptic and antiseptic. The cotton wool acts as a germ filter,

as well as an aspirator and an elastic medium of equable compression. Let us get at the facts before we speculate. Certain it is, that the perfect softness and elasticity of these absorbent materials are of the utmost practical value and comfort. Those qualities render possible, with the most perfect safety, the application of that elastic compression which, with rest and position, is so potent in relieving local congestion and inflammation. That therapeutic action is most clearly and convincingly demonstrated in cases of injury unaccompanied with wound. (Clinical Lecture by Mr. Sampson Gamgee, Birmingham, 1880, p. 8.)

Antiseptic Marine Lint.—My experience of it extends to nine or ten operation-cases, chiefly excisions of the breast. I have operated on these under the carbolic spray; have subsequently applied a layer of the "marine lint," moistened with carbolic solution (without any intervening protective), and two or three dry layers of the same over this; outside the whole, a piece of mackintosh and ordinary calico bandages. The cases were dressed again (always under the spray) on the third day; subsequently, at intervals of from five days to a week. All except one united without any suppuration, quite as cleanly and as rapidly as under the complete Listerian plan. In the exception (a case of excision of the breast for cancer), torsion had been used instead of the ordinary catgut ligatures; there was consequently a profuse serous oozing reaching the outer atmosphere; the mackintosh and amount of "marine lint" were not proportionately enlarged; and the method accordingly failed in preventing suppuration, though subsequently good recovery took place. For ordinary hospital purposes, this "marine lint" has two advantages over the Listerian gauze; the first is, that it acts much more completely as a sponge, soaking up and holding innocuous in its meshes a large quantity of discharge; the second, that it is very much cheaper—the expense being 2d. or 3d. per dressing, in comparison with from 1s. 6d. to 2s. 6d. in the other case. (Dr. H. L. Snow, p. 162.)

Dry Cotton-Wool Permanent Dressings in Amputations.—How much better is a patient when he can be left entirely alone for days or weeks. Unfortunately, this is often impossible or undesirable with the present modes of drainage. Though it is no longer necessary to meddle with wounds in order to pull away ligatures (thanks to Lister's invaluable absorbable catgut—a very great step in the direction of rest), still there is the drain-tube in the wound as a foreign body. For this reason, we are obliged to take down any dressing which includes it, in order to shorten or remove it altogether, no matter whether

the dressing itself could be left on otherwise or not. Could this be avoided, it would be most desirable. And it appears possible that it may be so in the future. In Langenbeck's Archiv, there is a very interesting article by Dr. Neuber, on the use of soluble drainage-tubes. He relates his experience with such tubes in a number of cases. They were made of decalcified bone, which he had found by experiment to be the best material for the purpose. Using these, he was able to leave a Lister's dressing, with some extra packing of salicylated wool, undisturbed for fifteen days, when the tube was found to have been absorbed, and its track healed. Now, if these absorbable drain-pipes can be combined with the catgut ligature and suture, we may perhaps be able hereafter with care to put up an amputation-wound once for all in a pure, dry, absorbent material, and leave it so until it is completely healed before removing the first application. This, if achieved, would go near to realise our ideal dressing, combining cleanliness, adequate drainage, and complete rest. Let us glance now at the dry cotton-wool permanent dressing as a method of wound treatment, and see how much of these three factors it contains. Knowing what cotton wool often is in hospital, where it is left about here and there—namely, a trap for dust and dirt—I have had all that employed here baked at a dry heat of 300° to 400° F. to render all organic matter in it innocuous, and destroy all germs. Now, pure dry cotton wool is well known to be a perfect shield against the access of dust and germs to substances covered by it. Familiar experiments go to prove this, and that air is completely filtered of all decomposition-exciting impurities by passing through it. If this be true, a wound perfectly clean in the first instance, carefully enveloped in perfectly clean cotton wool in sufficient amount to absorb its discharges, ought to remain quite free from contamination from without until the dressing is removed. The wool, so cleansed, was laid out just before the operation on a towel, to the depth of about six inches, ready to be slipped without loss of time under the stump, and lapped over the end and along its upper surface as soon as the sutures were completed. It was broad enough to envelop the limb, and was firmly bound round the latter with a bandage for a long distance up. Measures had been taken that the wound itself should be put up within the wool in a really clean state. First, the hands of all participating in the operation were well washed in a five-per-cent. carbolic acid solution. The whole limb both above and below the seat of operation was similarly treated, as also all instruments and appliances used. When this was completed in the usual manner, and the vessels secured with

carbolised catgut, the Esmarch's bandage was removed. The wound was then sponged with iced carbolic solution, perhaps, I since think, with unnecessary energy on my part. After this, I wiped it over in the three first cases with five per cent. of carbolic oil before stitching it with antiseptic silk and inserting an ordinary drain-tube. A strip of lint about as broad as the limb, dipped in the same oil, was then laid along the under surface of the stump, over its end, and along its upper surface. Over this, the wool was now quickly folded as described. (Mr. A. E. J. Barker, p. 177.)

Simple Method of Dressing Stumps.—Suppose an amputation of the thigh. After the operation is completed and the drainage-tube inserted, the stump is laid upon a Gooch's splint, padded and covered with gutta-percha tissue, and fastened to the splint by a turn of carbolised gauze bandage; the end of the stump and the drainage-tube should project a little beyond the end of the splint. The patient is then laid on a mattress, about one-third of which has been cut away on the side corresponding to the amputated limb. The stump is then swung from a cradle with the point dependent, and under it a plate containing a strong solution of carbolic acid or chloride of zinc receives the blood or other discharges as they fall from the wound. The cradle is covered with a veil of gauze. Thus no textile fabric is in contact with the wound, and as the stump is slung, all discharges drain away at once into the plate, and are there acted on by the antiseptic fluid. As no manipulation beyond lightly touching the cicatrix with a moist camel-hair pencil occasionally is required, perfect rest is ensured. (Prof. Spence, British Medical Journal, March 20, p. 436.)

AMPUTATION AT THE SHOULDER JOINT.—*New Method for arresting Hemorrhage.*—Lay a piece of calico bandage across the chest and upper part of the shoulder, and then fix an india-rubber cord or tourniquet round the shoulder over the bandage; this effectually compresses the axillary artery. In order to prevent the india-rubber cord from slipping, an assistant takes both ends of the bandage and holds them across the chest. If called upon to repeat the operation, I would pass a calico bandage under the india-rubber cord, *behind* as well as in front of the shoulder, then tie the four ends together, and thus dispense with the help of an assistant. (Dr. E. Moore, Lancet, Nov. 29, p. 796.)

AMPUTATION AT THE HIP-JOINT.—*Arrest of Hemorrhage by means of Davy's Rectal Lever.*—In an interesting discussion at the Clinical Society of London, following a paper read by

Mr. A. Pearce Gould, the use of the rectal lever was received with favour, and a comparison was drawn between it and digital compression. I can only state that, after a surgeon has proved practically how very easy it is to prolong indefinitely compression by the lever, he will not care to submit to the fatigue, uncertainty and anxiety of the digital pressure. On anatomical grounds, the situation of the common iliac artery is perfect for the ends of compression; the lever drops between the psoas magnus muscle and the bodies of the lumbar vertebræ, having the spring (sacral margin) of the true pelvic brim as a counter-resistance to the lever, and no large nervous trunks in the way. (Mr. Richard Davy, p. 188.)

ANTISEPTIC DRESSING OF WOUNDS.—I apply the dressing thus: The protective, dipped in the 1 to 40 watery solution to destroy the septic property of any dust adhering to it, is applied over the healing wound. Immediately over this is placed a piece of the carbolic gauze, dipped also in the lotion, to ensure that it is actively antiseptic when applied, for the dry gauze cannot be trusted in this respect, and over this the regular gauze dressing of ample dimensions. Remember the immense importance of having the protective overlapped well on all sides by the gauze. The protective, having nothing antiseptic in its composition, must be overlapped just as if it were itself a wound. The attainment of the results depends not only on the efficacy of the antiseptic measures used, but also upon the use of an efficient protective, as we term it—that is to say, a layer of material employed to protect the healing part from the irritation of the antiseptic itself. And with reference to this point, I may mention that we have now a considerably better protective than we had some time ago. It still consists essentially of oiled silk, varnished on both sides with copal varnish. The copal varnish is the principal agent in this protective for keeping the carbolic acid out; it is much less permeable to carbolic acid than oiled silk is, although oiled silk itself is much less permeable to the acid than gutta-percha or caoutchouc is. The copal varnish, then, is the most important of the ingredients of which this protective is composed. When it comes to be of the temperature of the body, it becomes perfectly soft and pliable; it applies itself beautifully to the part, so that there is no objection to it on that score. If we had not used a pretty effectual protective, we should not have had the kind of results you have seen in this case. If we had had a carbolic acid dressing acting directly on the tissues of that exposed

wound we should have had before now granulation and suppuration—suppuration occasioned by the irritation of the antiseptic—"antiseptic suppuration," as I have termed it; and if we had had the gauze dressing directly applied to that piece of dead tissue, it would have been impregnated with the pungent antiseptic, and would have become chemically stimulating on the same principle, though in a less degree, as if it had been putrid; it would have stimulated its living neighbours and urged them to granulation and suppuration, and we should have had a piece of dead tissue already separated by antiseptic suppuration. Again, if we had not used a pretty efficient protective, we should not have seen the blood-clot organised and cicatrising without suppuration at the edge of the window in the skin. We should have seen it with a granulating and suppurating margin. I believe the protective is as yet imperfectly understood. (Prof. Lister, p. 140.)

Carbolic Gauze as an Antiseptic Dressing.—We have improved upon this gauze of late. The proportions we used of the ingredients were originally one part of carbolic acid to five of common resin, and seven of paraffin, the paraffin being added to prevent undue adhesiveness. We have now changed these proportions to one of carbolic acid, four of resin, and four of paraffin. By that means we have in the first place a gauze dressing with half as much carbolic acid again in it. It turns out that this is not too irritating, and therefore that is a great improvement; for the gauze is of course more efficacious antiseptically in proportion as it has more carbolic acid in it. From the diminution of the paraffin we have a little more adhesiveness, but that, I think, is positively an advantage. We do not find it causes any serious inconvenience, and, on the other hand, it tends to prevent the disposition of dressings to slip upon the skin; it helps to keep them more securely in place. And then I may mention as a secondary, though not insignificant advantage, that though we thus increase so considerably the proportion of the carbolic acid, which is the most expensive ingredient, we have not added to the cost of the materials as a whole, because pure paraffin is so much more expensive than resin, that by diminishing the proportion of paraffin to the resin, we have cheapened the article more than we have enhanced its price by increasing the quantity of carbolic acid, so that the gauze is rendered slightly cheaper by this alteration of our proportions. The gauze has been employed in the usual manner in eight layers, with a piece of mackintosh under the outer fold; but in a case like this (empyema) we have also used the gauze in large amount in the form of loose-folded pieces underneath.

In the earlier stages this is of great importance, because the flow of serum that takes place from the unsupported pleura in the earlier stages is exceedingly copious; therefore we have made the mass of gauze more than usual. In the adult it becomes necessary to change the dressings twice a day in these cases, but in this child the discharge being of course much less, once in the twenty-four hours proved sufficient. (Professor Lister, p. 105.)

Lister's Antiseptic System.—Carbolic Acid.—Up to the present time Mr. Lister has found carbolic acid the most efficient agent for carrying out his system. Of the several varieties of acid in use, he recommends *absolute phenol*, the granular form of which seems the most convenient to work with. The reason for this preference is the purity of the substance, which forms clear lotions, free from all unpleasant odour, and less irritating to the skin of the patient and the surgeon's hands. Should a cheaper variety of acid be used, on economical grounds, the No. 3 acid seems the most suitable, but it is advisable that its solutions should be filtered through several ply of muslin. Though carbolic acid is termed an *acid*, there is chemical evidence in favour of its partaking of the characters of an *alcohol*, and hence the term phenol, which it has obtained under this latter view, is synonymous with the more general one of carbolic acid. For practical purposes, the readiest test of the purity of carbolic acid is its solubility in water, and this is indicated by its pungency, and by its effect on the skin, a pure 1-40 solution being scarcely, if at all, inconvenient to the hands. The carbolic lotions in use in the antiseptic system are two *watery* ones, two *oily* ones, and a *spirituous* one, for a special class of cases. The urine of patients dressed with antiseptic dressings may become blackish in colour on standing. This probably indicates the presence of an increased amount of the indican or colouring matter of the urine due to the presence of carbolic acid in some form in the blood. The most convenient test for carbolic acid in the urine is the ordinary commercial sulphuric acid. (Dr. G. Beatson, p. 149.)

Boracic Acid, Salicylic Acid, Chloride of Zinc, and Iodoform.—In addition to carbolic acid, Mr. Lister has introduced into his system the following substances, boracic acid, salicylic acid, chloride of zinc, and iodoform. Boracic acid is a bland and excellent antiseptic, but being non-volatile is used chiefly in superficial wounds. It is employed in three forms —(*a*) a cold saturated solution, (*b*) lint, (*c*) ointment. In antiseptic cases, dressed with the carbolic gauze, where any eczematous eruption appears in the vicinity of the wound, it

is advisable to moisten the "deep dressing" or the inner layer of the gauze with the 1-40 carbolic lotion, thickened with powdered salicylic acid. In those rare cases where carbolic acid poisoning makes its appearance under the use of the ordinary antiseptic dressings, salicylic acid may be exchanged for the carbolic acid and employed in the form of salicylic jute. The only solution of chloride of zinc in use in Lister's system is one of 40 grains to the ounce. This chloride of zinc solution is suitable for correcting putrefaction in superficial wounds and sinuses, and is advantageous as an application to recent wounds in the vicinity of the different cavities of the body. Iodoform, in the form of powder, is sprinkled over ulcers, so as to purify them previous to applying antiseptic dressings. (Dr. G. Beatson, p. 149; and Glasgow Medical Journal, Jan. and Feb. 1880.)

DISEASED JOINTS.—*Anchylosis.*—As regards healthy joints, there does not exist a single fact which shows conclusively that fixation, however long continued, has ever led to anchylosis. This long-continued fixation may, it is true, give rise to anatomical modifications such as diminution in the extent of the articular surfaces, to a thinning of their lining cartilage, also to a reduction in size of the synovial sacs, of a less abundant synovial secretion, and to functional changes, such as stiffness of the joints and limitation of movements. Hence, not unnaturally, when the necessity for immobilisation has ceased, a certain time will be required for the complete restitution of the articular function. But there is nothing in all this which at all resembles anchylosis. The exaggerated fear of anchylosis has caused many practitioners to make grave errors, and has frequently led to the too early leaving off of passive fixation, and the too premature recommencement of movement. (Prof. Verneuil, p. 182.)

GENU VALGUM—*Subcutaneous Osteotomy in Young Children.*—The operation of subcutaneous osteotomy was first proposed by Dr. A. Ogston of Aberdeen. Great praise is due to Mr. Reeves for having improved on Ogston, by suggesting a subcutaneous osteotomy; and further, one which may frequently be done without even entering the joint. Reeves' modification seems to me a less serious operation, and just as effective. Since its introduction, therefore, I have given up all other operations for uncomplicated genu valgum. In children, I believe it can be performed without entering the joint, for the layer of encrusting cartilage, together with its synovial lining, would stretch rather than crack sharply off, as was probably the case in adults. The operation is performed thus: An incision is made with a small scalpel down

to the condyle. It should be made about its centre from before backwards; this will correspond to the axis of the femur, and will be about midway between the tendon of the adductor magnus and the inner margin of the patella. The chisel is then introduced, and the condyle is gradually separated. It is well to avoid entering the condyle too far in any one given place; the attempt should rather be made to chisel it equally in each direction by making the handle of the chisel describe the arc of a circle while the mallet is being gently used. If the chisel be sharp, a mallet will not be necessary when operating on young children. After the condyle has been freely loosened, *redressement forcé* is practised until the deformity is corrected. A little over-correction even is desirable. Then plaster-of-Paris bandages are applied from the toes upwards, and the limb is held in position until the plaster is hard. An ice-bag is kept applied for three or four days. There is not much pain subsequently, and rarely any constitutional disturbance whatever. After an interval of from four to eight days, the plaster bandages may be removed, and also the Lister dressing. It will generally be found that the limbs can be *voluntarily flexed to about two-thirds of their extent immediately afterwards*. The amount of stiffness varies with the length of time which has elapsed since the operation. If the joint have not been entered, there will scarcely be stiffness at all. But under any circumstances, I would dissuade from any movements but those of which the patient is voluntarily capable. I should not propose to allow the patient to walk about so soon as this, as it is impossible for the process of union and repair to have been completed thus early. In some of my earlier cases I waited for three weeks, in others for a fortnight, before commencing movements. I have gradually reduced the time, as experience has shown it to be possible. *Voluntary movements* are always to be preferred, and should be practised every day. (Mr. R. W. Parker, p. 206.)

Operations on the Mouth and Throat.—*Position of the Head in.*—It is an excellent plan in operations on the mouth and throat to allow the patient's head to fall over the edge of the table. Although the tongue falls back towards the posterior wall of the pharynx after its attachments to the jaw have been freely divided, the breathing will be found to be perfectly easy, much more so than when the head is raised. I have performed many operations in this way on the mouth and throat with complete success, and with great facility as regards the prevention of blood passing into the air-passages. My present method of keeping the head in this position is to

have it hanging over the end of the table and supported there by the hands of an assistant. (Prof. Annandale, p. 211).

SPINAL DISEASE.—*Method of applying a Plaster-of-Paris Jacket in the Recumbent Posture.*—The following method is a modification of Dr. Walker's of Peterborough. The best material for the bandages is Victoria lawn, but not too close in texture; and four yards is a convenient length for them. The plaster should be well rubbed into the bandages, but it is desirable to have a considerable quantity besides loose on the surface of the bandages. Shortly before the jacket is to be put on, the patient should have a full meal, thus putting "a pad" of the correct size inside, instead of Sayre's "dinner-pad" outside. The patient, having put on a thin closely-fitting woollen shirt, is laid on his back in bed, or on a table covered with a folded blanket, a waterproof under him, and his arms extended at right angles to his body. The requisite depth of jacket is marked on the bed by any appropriate means, such as pins inserted in the waterproof or blanket, two on each side, one opposite the axilla and another opposite a point about an inch below the crest of the ilium. The patient is now made to sit, and after doing so must be careful not to change his place on the bed, in order that, when he again lies down, he may resume exactly his former situation. The length of bandage having been ascertained, which is requisite to encircle the body and overlap in front to the extent of four or five inches, pieces of bandage cut to this length are laid down in succession across the bed, each piece overlapping the preceding by two-thirds of its breadth, as Dr. Walker directs, until a sufficient number are laid down to give the required depth of jacket. The first and the last piece laid down should, however, be about a third of their breadth beyond the marks, and should be then folded in, as otherwise the jacket at its upper and lower edges would be only twofold. The ends of the bandages are now fastened together by being fixed on each side between two slips of wood, which are clasped by three American clothes-pins. These slips of wood should be about fourteen inches long, one inch broad, and three-sixteenths of an inch thick, and the wood should be left in its rough state on one side, in order to grip the bandages more firmly. If any of these were to slip out after being wet, the whole operation would be spoiled; this could be effectually guarded against by one of the pieces of wood being pierced by a row of very small sprigs about an inch apart, so that their sharp points might project slightly on the rough side. I have not found this necessary, being careful when lifting the bandages to hold firmly with the

fingers. By this means, the whole layer of bandages can be lifted between the two hands and dipped in fluid plaster; but before doing so, a second layer of bandages must be made ready in the same manner. No water having been as yet employed, all these arrangements can be gone about with perfect leisure, and it may be well to cause the patient to lie down on the arranged bandages to make sure that the correct size has been taken. (Dr. J. W. Miller, p. 208.)

AFFECTIONS OF THE SKIN, ETC.

ACNE ROSACEA.—Dr. S. Eldridge treated an obstinate case of acne rosacea, occurring on the nose of a young lady, by the use of ergotin, applied during the night upon lint; in three weeks there was much improvement, and in six months no trace of the disease was visible. In another case of the same affection, due to drinking, he injected two or three minims of the following preparation of ergotin into the substance of the skin at intervals of three days, having first softened the tissues as much as possible by several days' continuous poulticing:—Ergotin, gr. xv.; glycerine, gr. xxx.; water, ʒ ii.; thoroughly triturate and strain. No suppuration occurred. Thirty injections were made, and in four months the nose was almost natural in appearance. (New York Medical Journal, Oct. 1879.)

BALDNESS.—*Kaposi's Treatment.*—The active agent employed is the German or French soft soap in alcoholic solution, which, while it is slightly stimulant, removes the scales and thoroughly cleanses the scalp. ℞. Saponis viridis (Germ.), alcoholis āā. ʒ ii. Solve, filtra, et adde ol. lavandulæ gtt. xx-xxx. This makes a very handsome orange or wine-coloured preparation, with a pleasant odour. It is used as a shampoo every morning or evening, pouring one or two tablespoonfuls on the head. On the addition of water, and smart friction with the fingers, a copious lather is soon produced. After keeping up the shampooing process for four or five minutes all the soap must be washed out of the hair by the free use of warm or cold water, and the hair thoroughly dried with a soft towel. The immediate effect experienced is a disagreeable feeling of tension of the scalp, as if it were stretched too tightly over the skull; this is relieved, and the scalp prevented from becoming too dry, by some fatty application, the best, as well as the neatest preparation which can be used for this purpose being cosmoline, a hydro-carbon obtained from petroleum. Cosmoline is perfectly bland and unirritating, never turns rancid, and is comparatively cheap; it may be

obtained in the fluid form or as a soft solid. This procedure must be repeated daily for three or four weeks, when it will generally be found that the production of scales and the falling of the hair have been very markedly decreased. (Dr. G. H. Rohe, p. 299.)

BURNS AND ULCERS.—*Healing by Union of Granulations.*—In cases of extensive superficial burns, it is a good plan to draw the edges together, and for this purpose to employ the button-suture. Having washed the sore carefully with boracic lotion, I operated under the spray, because, after the parts are drawn together, the wound ceases to be a superficial ulcer. I then entered the needle two inches from the sore, and brought it out at the margin, entered it again at the opposite margin, and brought it out two inches off. When the sutures had been drawn tight upon the buttons (of the largest size), the sore, originally three inches in diameter, was reduced to a line three inches long. It was then dressed with carbolised gauze. The dressings were changed on the fourth day, and, on the ninth, I removed the buttons, the sore being healed with the exception of little marks where the silver wire passing from the buttons had slightly cut the skin. The buttons should be of the largest size, at least an inch long; and I have found the greatest benefit from the little projections on the side which Mr. Lister has recently added. (Mr. John Duncan, p. 280.)

ECZEMA.—*Oleate of Lead.*—One of the best local remedies for eczema is the ointment of oleate of zinc, for which the profession is indebted to Dr. Crocker. Some time ago, with the view of testing its action in the treatment of eczema, I desired Messrs. Southall of Birmingham to make for me an ointment of oleate of lead. After a series of experiments they produced an excellent ointment according to the following formula:—Lead oleate, 24 parts; heavy and inodorous paraffin oil, 14 parts. The lead oleate is prepared by heating a mixture of oleic acid and oxide of lead. (Dr. J. Sawyer, p. 287.)

Chronic Eczema.—*Oxide of Zinc.*—In one case I ordered two or three different applications each day to different parts, with the hope of finding something to mitigate the burning pain. One happened to be the oxide of zinc, four drachms, rubbed up with eight ounces of water, adding one drachm of hydrocyanic acid (Scheele's); the other two the woman said aggravated the pain, when this gave instant relief, and, in fact, her pain of weeks was suddenly, as she said, cured. I have tried the same lotion, and without the hydrocyanic acid, in many cases since with the most wonderful relief, and which

can be improved on by bandaging oiled silk over lint soaked in it on the legs and arms. (Dr. H. Fisher, Lancet, Jan. 3, p. 39.)

Chronic Eczema of the Palm.—The following lotion I have found very beneficial in allaying the intense irritation which so often accompanies this common skin-disease. It consists of bicarbonate of soda, 2 drachms; bicarbonate of potash, 1 drachm; glycerine, 1½ drachms; tincture of opium, 2 drachms; water to 8 ounces. I was at first induced to use it in a case of eczematous irritation, in which every application I had used proved of no avail, on account of the marked relief from pain which the local use of the bicarbonate of soda frequently gives in severe burns. Only a few days ago in the case of a lady suffering from chronic eczema of the legs, accompanied with intense itching, I used it with the most beneficial and immediate results, the patient showing me some half-dozen prescriptions which she had used with little or no success. I consider the bicarbonate of soda lotion almost, if not quite, a specific for the relief of the intense burning irritation which often attends chronic eczema, more especially if the patient have a rheumatic tendency. (Dr. J. W. H. Lush, British Medical Journal, Nov. 22, p. 814.)

PORT-WINE MARKS.—*Obliteration by Linear Scarification.*—The details of my new operation are simple. At the first operation, I divide the skin into small squares, by two series of parallel incisions, the one set crossed at right angles to the other, just as in my former method; but the incisions, instead of entering the skin perpendicularly, enter it slantingly. I may call the one series of parallel incisions longitudes, and the other cross series latitudes. At my first operation, the obliquity of the longitudinal incisions is directed eastwards, and that of the latitudinal one southwards. At the second operation, executed about a week afterwards, the lines of longitude and latitude are re-executed still in the same directions respectively, only that now the obliquity of the longitudinal incisions is directed westward, while that of the latitudinal ones is directed northward. In this way, the obliteration of a port-wine mark may be effected in a fortnight if the whole of the mark be operated on at once, although it may often be more convenient to treat the mark piecemeal. (Dr. Balmanno Squire, p. 298.)

PRURITUS.—*Balsam of Peru.*—Dr. Auerbach, of Berlin, states that having, in common with so many other practitioners, found the balsam of Peru a most valuable remedy in itch, he has for some time used it in the treatment of pruritus with the greatest success. After the first rubbing into the part

affected, great relief is obtained, and in a few days a cure results. He relates a very obstinate case which, after resisting all kinds of treatment for years, was speedily cured by the balsam. (Dub. Jour. of Med. Science, March, p. 192.)

PSORIASIS.—Arsenic should always be begun with in small doses —generally three or four minims of Fowler's solution diluted in water—giving it quite regularly three times a day after meals. It may have to be continued perhaps for months, and it is rarely necessary to increase the dose, which indeed, small as it is, may give rise to swelling and itching of the eyelids, pains in the head, gastric disturbance, &c. When this is the case, the dose must be diminished or the medicine suspended. As to local treatment, where the eruption is of recent date, and running an acute course, soothing applications must be used. In these cases there is a constant exfoliation of scales which must be first removed by plain or alkaline baths, a very good one being made by dissolving a pound each of borax and bicarbonate of soda in thirty gallons of water. After the bath the patient should be anointed with cosmoline or vaseline, and this treatment should be continued until the eruption fades away or becomes sufficiently chronic to allow more stimulant applications. In the chronic stage, Prof. Maynard thinks highly of chrysophanic acid, fifteen grains to the ounce of lard. It should be applied with great care only to the patches of the eruption, as in some it produces great irritation of the skin. An objection to its use is the peculiar staining of the skin which it produces; and on this account it is best to employ it on parts covered by the clothing, substituting for it on the face and hands the sapo viridis, or German green soap, which may be used alone, or, if found too irritating, with one or two parts of alcohol. Pyrogallic acid (in a 10 per cent. solution in water) has been recommended as a substitute when the chrysophanic acid cannot be tolerated, being much slower and milder in its action. Liquid pitch, or oleum cadini, may also be used with good effect, either as ointment with lard or cosmoline, or in lotions with alcohol or eau-de-cologne. Usually the disease will yield to these means, but occasionally it proves very obstinate and very liable to relapse, especially in winter. In such cases the arsenic treatment should be continued for several months after the subsidence of the eruption. (Prof. Maynard, p. 289.)

Pyrogallic Acid.—M. C. Mook (in the service of M. Vidal) uses this remedy in the strength of 1 part to 10, and states that it never produces the inflammatory phenomena which so often follow the application of chrysophanic acid or oleum

cadini. It does not discolour black hair, although it turns blonde hair brown, and is destitute of odour. (Practitioner, April, p. 295.)

Preismann calls attention to the urgent necessity for removing the scales before applying a remedy to the diseased spots, and speaks of the difficulty of doing this thoroughly. For this purpose he was led to use, and now highly recommends, a solution of salicylic acid in alcohol, one part to sixteen. When this solution is rubbed lightly on a patch with a cotton swab, the scales instantly become loose, and fall off, leaving the patch red, dry, and even. It also removes the grease and renders the action of the subsequently applied medicine much more efficacious. The lotion acts very efficiently in relieving the itching, and he has found it useful in chloasma, lentigo, &c. (Glasgow Medical Journal, Nov., p. 392.)

RINGWORM.—The best remedy to prevent its spreading amongst children, when they are obliged to associate together, is the carbolised glycerine of the British Pharmacopœia, either pure or diluted with a little more glycerine; it should be well rubbed all over the scalp every morning. This acts in two ways: (1) The carbolic acid produces its usual effect on organised matter; and (2) the glycerine prevents particles of scurf from being dispersed. *Strong remedies are always contra-indicated in very young children;* a little tincture of iodine painted on once a day for a very few days, followed by the use of the white precipitate ointment, is all that is necessary. In older children stronger treatment must be used, but even then you must be guided in your choice by the extent of the mischief. It is very unwise to make a *large* sore place on the scalp, as it will very likely give you and your patient more trouble than the ringworm itself. If, however, the disease is in an early stage, and consists of one or two small circumscribed spots, your best plan is to cut the hair short all round the spots, and apply with a brush Coster's paste, acetum cantharidis, or iodine liniment. At this stage a few applications will sometimes arrest the mischief. A single painting with pure carbolic acid is thoroughly effective, but it is a strong remedy, and gives some pain. Always bear in mind that it is very unwise to trust strong remedies to unskilled hands. When the disease extends over a large surface you must be content with using milder measures—tincture of iodine of double strength painted on every day is a good and safe mode of treatment. This may be followed up by the use of the nitrate of mercury ointment, diluted according to circumstances, or an ointment containing the red

and white precipitate of mercury and sulphur, or the oleate of mercury (10 per cent.) For many years I have used in certain cases goa powder or chrysophanic acid ointment (thirty grains to the ounce is usually strong enough), and I have found it a very effective remedy, but there are great drawbacks to its general use. (Dr. R. Liveing, p. 291.)

Ordinary Recent Ringworm.—An excellent remedy is a mixture of carbolic acid (the pure crystals melted) and glycerine, in equal proportions, or one part of carbolic acid to two or three parts of glycerine, according to the extent of surface to which it is applied, the age of the patient, or the effect produced. The glycerine penetrates freely, and carries the acid to the conidia. It is cleanly, seldom painful, and though I have used it most freely the last eight years, I have only once seen any toxic effect produced, and that was when applied for some time to the entire shaved scalp of a child about five years old. It is best to cut the hair very short with scissors all over the head, or to shave it, except, perhaps, a little fringe back and front, to show under the cap, if the patches are at all numerous, or half cover the scalp. In other cases it is sufficient to cut the hair off the patches, and for half an inch round them. Then, if there are only a few patches, I blister them freely with liq. epispasticus, and after a day or two begin to apply the carbolic glycerine. Blistering large surfaces is very painful, and not without risk, and, in my opinion, is quite out of the question with young children; in fact, carbolic glycerine soon cures such cases. If the ringworm be so extensive as to require most of the hair to be removed, the carbolic glycerine may be applied to the entire scalp, as strong as it can be borne without causing much pain or blistering; but it is necessary to exercise more caution, using a weaker solution in the case of young children; and it is also advisable to apply it only to a portion of the scalp the first time, and to note its effect. In employing this remedy, the most important points are to apply it often, say twice or thrice a day, and to have it well rubbed into the places with a sponge mop. Nurses generally shirk this duty; therefore, the doctor must take care that this preparation is properly applied. It will take two or three months at least of this treatment to produce a complete cure, and to get the new downy hair to grow. At the same time, if the remedy be only applied to the patches, a weaker solution, say 1 in 8, must be well rubbed into the entire scalp at night, to insure the destruction of any conidia that may be about, and to prevent the extension of the disease. During the first part of the treatment, the hair should be

kept close cut, and any small patches of ringworm should be painted with strong carbolic acid and glycerine (3 to 1 of glycerine), blistering fluid, or glacial acetic acid. The scabs produced by these applications should be removed with forceps as soon as formed—often bringing away with them many diseased stumps—and the remedy again used. Where there is a large extent of surface involved, I have used the undermentioned ointment the last year very freely, with excellent results—viz., carbolic acid (pure), strong citrine ointment and sulphur ointment in equal parts. To be made fresh every week. This can be applied without fear, at any rate to children over ten, to the entire scalp every night, and to the patches again the next morning. In children under this age it is advisable to use a double proportion, or even more, of the sulphur ointment. It causes no pain, and is very effectual. Of course, in accordance with the rule mentioned above, the head will not have been blistered. A cap should be worn as before described, and the head well washed twice a week, all scabs being removed from the patches. The scabs will turn of a blackish colour, especially if the ointment be used after being made a week, as a sulphide of mercury forms. While using this ointment all over the scalp, I often apply strong carbolic glycerine to the small patches. I have sometimes noticed that this preparation, after being used for a while, causes the stumps to become loose in the follicles, so that they can easily be extracted with the roots. I may mention that this ointment often turns the hair of a light golden colour. (Dr. Alder Smith, Lancet, Jan. 10, p. 54.)

Chronic Ringworm.—The difficulty is not to find some parasiticide that will destroy the ringworm fungus, but to bring the remedy into contact with it. In recent ringworm the conidia only penetrate a certain distance into the follicles, and therefore the disease is easily cured by almost any remedy; *but after a time they penetrate to the very bottom of the follicles*, which it must be remembered are even below the true skin, and into the roots of the hairs. Therefore it is impossible to reach the fungus by any ordinary remedy applied to the surface of the scalp. Again, the stumps are so diseased that they cannot be extracted by epilation, but always break and leave the root part swarming with the fungus down to its very base. One must therefore select a remedy that will penetrate deeply into the follicles, and Mr. Marshall's preparation of oleate of mercury is certainly the best for this purpose, combined afterwards, if necessary, with the treatment I will call "the artificial production of kerion." It is prepared as a thick solution in

oleic acid, and sold as a 10 per cent. and 5 per cent. solution. I use the 5 per cent. for children under eight years of age, and the 10 per cent. to those over that age, if the skin will bear it without much irritation. It is advisable to have acetic ether mixed with it in the proportion of one part of ether to seven parts of oleate. This increases the penetrating power. The mixture must be kept in a stoppered bottle. The treatment is commenced by having the hair cut short, about a half to one inch long. The head should then be thoroughly washed with soft soap and water, and when dry *the oleate should be rubbed well into the entire scalp*, if the disease be at all extensive, with a small sponge mop. This must be done regularly, especially into any patches, night after night, and again in the morning into any well-marked diseased places. The caps before described should be worn at night, special care being taken that the oleate does not run on to the face or pillow-case, as it will produce much irritation, and even a rash. It is advisable to apply a thin towel, like a turban, round the head, as well as the cap, to prevent any oleate getting on to the face. It is most important that the head should not be washed more than once a fortnight. If water be constantly used, it tends to prevent the oleate from soaking to the bottom of the hair-follicles, which is so essential to the success of this remedy. Therefore, while the case is under treatment, which may be from three to six months, wash well every fortnight, cutting the hair again if necessary, removing all the scabs and yellow incrustations with the point of a pair of forceps, small-tooth comb, or other means, and then examine the scalp with a lens, to see how the case is progressing, before applying the oleate again. Of course care must be taken that no mercurial symptoms arise during the treatment. If the 10 per cent solution produces too much irritation, the 5 per cent. must be substituted. It is important to obtain the oleate from a first-class chemist. (Dr. Alder Smith.)

With regard to *kerion*, it should be remembered that the artificial production of it should only be attempted in old chronic small patches that have resisted all other treatment for months or years; that it should be produced by the daily application of croton oil, together with constant poulticing; that croton oil must be used with very great caution till the hairs get loose in the follicles, the great aim of the treatment being to cause inflammatory swelling of and effusion into the tissues around them; that the stumps must be removed time after time as they appear; and that great care must be taken to prevent the oil getting on to the face. Lastly, with regard to the *disseminated variety*, it is best treated by touching each

stump with croton oil, week after week, except perhaps in very exceptional cases, where a minute quantity of the antimonial paste may be required. (Dr. Alder Smith, Resident Medical Officer at Christ's Hospital, London, Lancet, Jan. 24 and 31, pp. 126, 164.)

SCABIES.—A very common error occurs in the following way. A patient suffering from an eruption consults a medical man, who believes it to be the itch, and orders accordingly the sulphur ointment of the Pharmacopœia to be well rubbed in; the patient, wishing to get rid of his trouble as fast as possible, carries out the treatment very vigorously; he is probably much better after the first and second application, but he soon begins to get, as he thinks, worse; the eruption extends and becomes more copious, and the more he applies the ointment the more he suffers; after a time, he returns to the medical man, who finds him covered with an irritable eczematous eruption; and, as he now fails to find anything distinctive of itch, he is apt to think that he has made a mistake. But the fact is that he has probably made no mistake, except that of prescribing a very strong ointment, which his patient has used too freely, and thus, while he has cured the scabies, he has produced a great amount of artificial eczema by the process of cure. Of all remedies, not one is so effective as sulphur ointment properly applied. An ointment half the strength of that of the British Pharmacopœia is quite strong enough, and the best time to use it is at night, when it should be rubbed all over the body, except the head, but especially on the hands, buttocks, and lower part of the abdomen, and then the underclothing used during the previous day, namely, socks, gloves, drawers, and jersey should be worn during the night; this thoroughly disinfects the clothes, and at the same time keeps the ointment well applied to the skin. In the morning, a warm bath may be taken, and no treatment followed during the day. For three nights the process should be repeated, but never longer; subsequently a little ointment should be well rubbed on the hands, wrists, and buttocks for a few nights. All treatment should then be discontinued for at least a week, when, if necessary, it may be repeated for one or two nights, or a milder ointment might be used. It is sometimes difficult to say whether a case of scabies is cured or not; under these circumstances it is very convenient to use an ointment which does not irritate or annoy the patient by its disagreeable smell, and which at the same time will complete the cure. A most excellent ointment of this kind is made with balsam of Peru (ʒ ij ad ʒ j). The styrax ointment is also thoroughly effective, but less agreeable. (Dr. R. Liveing, p. 295.)

ULCERS.—*Martin's Rubber Bandages.*—In the out-patient rooms of our London hospitals there have been ample opportunities for testing the efficacy of these bandages, and I am sure sufficient experience has been obtained to warrant us in giving an opinion as to their utility. I have been surprised at the result. I must confess that there is some difficulty in keeping these patients under observation long enough to witness the ultimate success of the treatment—the perfect cicatrisation of the ulcer. They see no object in losing time by waiting to see the surgeon in the out-patient room after they have lost all pain, can stand all day, and their ulcers are diminishing in size. Consequently as they learn to treat themselves they discontinue coming, and we are unable to say how soon the ulcer heals, and to what extent the patient has been benefited should it not have healed. Now and then they return to show themselves, expressing the greatest gratitude for the relief they have obtained, and these are generally the worst and most chronic cases that we see in the out-patient room. They are simply astonished at the effect produced by such simple means after years of suffering and incapacity for work. Patients with ulcers of only short duration do not trouble themselves to return. To be cured at once of their troubles is nothing more than what they expect, at least this seems the most reasonable explanation of the fact that only the worst cases return to report progress. The treatment consists in applying a solid rubber bandage to the limb affected, before getting out of bed in the morning, without any application whatever intervening between the limb and the bandage. It is put on with only just sufficient degree of tightness to prevent its slipping down. It is kept on all day and only taken off at night on going to bed, when it is then washed in warm water, and hung up to dry. The ulcer is then covered with some simple dressing, which is kept in contact by a few turns of an ordinary roller. All greasy applications are best avoided, as they soon have a deleterious effect upon the rubber, unless very carefully removed in the morning before applying the bandage. All ulcers can be treated on this principle, but the varicose ulcer is the one more especially benefited by this method. The syphilitic ulcer, of course, requires the special treatment of the disease, but after a time the syphilitic ulcer often becomes as much a local disease as any other ulcer; the cure can then be completed by Martin's bandage. (Mr. H. H. Clutton, p. 282.)

Iodoform.—I have just dismissed from under my care, cured, a case of varicose ulcer that had defied the patent ointments

that are so much advertised, and had also been under the care of several medical men. Iodoform dusted over the sore twice a week, and a daily dressing of boracic acid ointment, soon made a change in its appearance; and in about fourteen days it healed, and the patient describes the limb as being quite free from pain—a thing quite new to her. A year ago, I had an old soldier under my care with an ulcer of the size of half-a-crown upon the outer aspect of the right leg, four or five inches above the ankle-joint. It first formed in the Crimea, during the first bombardment of Sebastopol, and followed a blow from a splinter. For some years he took little notice of it, merely keeping it clean. Latterly, he had dressed it with all the patent ointments he saw advertised. It was an ugly, unhealthy-looking ulcer, discharging freely, and was very painful both during the night and when walking. Its depth varied: in the centre, it was quite an inch and a half. Its edges were irregular. The leg was free from varicose veins, and the man was temperate. His occupation entailed much climbing, and he had the ulcer often struck by pieces of timber. I first dusted it with iodoform daily; in a week, every other day; and then twice a week. The effect was marvellous; for in six weeks the ulcer healed, and to this day continues well; the cicatrix is firm; and the man can follow his occupation and take long walks without any pain or inconvenience. I am certain that no other treatment would have healed this ulcer. I have found boracic acid ointment alone to do wonders with ulcers; but, with the addition of iodoform sprinkled on the sore, much more can be done. (Dr. W. Easby, British Medical Journal, March 6, p. 362.)

AFFECTIONS OF THE EYE AND EAR.

CATARACT.—In cases of infantile cataract, after opening the capsule widely, and leaving the lens to be softened in the aqueous humour, I remove it three or four days afterwards by puncture with a broad needle, which is introduced obliquely through the corneal substance, so that upon its withdrawal complete coaptation is preserved, rendering hernia iridis impossible. In cases of senile cataract, I make an iridectomy generally downwards, from two to three weeks prior to extraction. In a case of urgency there need only be an interval of a few days between the iridectomy and extraction, as the two incisions do not interfere with each other. The section of the iris is made subconjunctivally, with a lance of the size shown in fig. 1. Should

the iris not prolapse on the withdrawal of the instrument, it is drawn out with Tyrrell's hook, and its internal circular border is laid hold of with a small pair of iris forceps, and rounded off with scissors, in such a way as to imitate the natural pupil and leave the ciliary border untouched. In the operation for extraction, the patient being in a recumbent position, I introduce a speculum, and fix the eyeball by means of forceps tipped with gutta-percha, and with Graefe's knife I enter the external margin of the cornea close to its sclerotic junction; the counter-puncture is made directly opposite on the inner side, the points being so chosen that the corneal flap extends a millimetre more than the third of its circumference. The knife is then pushed in a plane parallel to the iris. When the corneal section is nearly completed I withdraw the knife. In cases of soft vitreous I open the capsule widely with the point of the knife before withdrawing it. I then remove the speculum. (2) With my thumb and index-finger serving the purpose of a speculum, I introduce the cystotome, and make sure that the capsule is properly opened; and in cases of soft vitreous, when the lens has a tendency to sink, I make gentle pressure upon the eyeball, and, using the cystotome as a crotchet, I bring the lens forward and put it in a position facing the corneal wound. (3) I introduce a small blunt-pointed corneal knife with a convex edge (fig. 2), which I direct obliquely to divide the corneal bridge. (4) After a few seconds of rest, I seize a fold of the upper eyelid between my fingers and depress the lower with my other thumb, and, directing the patient to look straight forward, I exercise pressure upon the lower part exactly in the vertical meridian facing the middle of the iris section, and, with the other fingers through the eyelid, I press upon the wound to make it gape, when the cataract advances to the corneal incision. Any *débris* remaining behind is easily extruded by gentle manipulation. (Fig. 3.) When the vitreous is soft I leave a larger corneal bridge, and instead of finishing the circle at the corneal margin, I carry it more forward into the corneal substance, so as to protect the vitreous from prolapsing. I avoid giving chloroform, and do not dilate the pupil previous to extraction, as then there is a risk of a part of the iris falling upon the knife. (Dr. J. R. Wolfe, p. 260.)

Senile Cataract.—At the South London Ophthalmic Hospital Graefe's method is chiefly followed. The essentials are, 1st. That a Graefe linear knife be employed; 2nd. That the section be made upwards; 3rd. That the section be wholly in the corneal tissue; and 4th, That there be an iridectomy.

The details vary slightly, according to the opinion of the operator or the nature of the case, that is to say, the section may be made at the extreme periphery of, or more anterior in, the cornea; the base of the flap may be higher or lower, according to the size of cornea or nature of cataract; and lastly, there are small differences in the method of excising the iris, lacerating the capsule, and removing the lens, not any of them sufficient to merit giving a different name to the operation. In performing the Brussels operation, the puncture and counter-puncture were made with a Graefe's linear knife at the periphery of the cornea on a level with the upper border of the undilated pupil; the edge of the knife being then turned forwards, the operator cut directly out through the cornea at a point midway between the upper margin of the pupil and periphery of the iris, the capsule being lacerated and the lens removed without an iridectomy. A solution of sulphate of eserine (four grains to the ounce) was usually instilled immediately to prevent prolapse of the iris. (Mr. A. Stanford Morton, Ophthalmic Hospital Reports, Dec., p. 375.)

Extraction of Semi-fluid Cataract by Suction.—Mr. Teale, who introduced this method into British Ophthalmic practice, thus summarises his matured views on the subject. 1st. When a cataract is almost fluid it will readily flow out in the groove of the ordinary curette, and there is no need of extraction by suction. 2. When a cataract is immature or semi-solid, to attempt to withdraw it by suction is to put enormous atmospheric pressure upon the ocular structures and to risk disaster. 3. For cataracts between these extremes—and they constitute a large proportion of soft cataracts—there is no method, to my mind, more simple, more speedy, more effectual, more safe, or less liable to be followed by opacity of the capsule, than the method of suction. 4. To use a suction instrument with safety requires attention to minute details, no less than is needed for the use of the lithotrite. Lithotrity carelessly performed becomes disastrous. Performed with due care and gentleness, it is one of the most successful of the major operations of surgery. (Mr. T. Pridgin Teale, p. 264.)

Chronic Conjunctivitis.—*Ergot.*—In chronic conjunctivitis, in which the vessels are enlarged and tortuous, Dr. Dabney advises the frequent cleansing of the eye with warm water, and the instillation after each washing of a few drops of the following solution:—Ergot (solid extract) gr. x., glycerine ʒ i.; water to make ʒ i. This treatment is less applicable in cases in which there is much pain or intolerance of light. In

pterygium the same solution may be used with advantage.—(Dr. W. C. Dabney, American Journal of Medical Science, July 1879.)

CORNEAL TRANSPLANTATION.—The cornea can maintain its vitality and transparency when transplanted from one place to another, but to be successful it must be taken from a freshly enucleated human eye. All the incisions in the cornea must be clean, as any tearing is likely to lead to suppuration, and the measurements of the graft, &c., must be exact. It must be done in such a manner as not to damage the subjacent structures. This is absolutely impossible if we attempt the transplantation of the whole of the cornea, because as soon as we remove it the lens and vitreous come out, bleeding follows, and loss of vision is inevitable even if we secure corneal transparency. But the removal of the whole cornea is not necessary, and the chances of maintaining its vitality decrease in proportion to its extent. Hence, if we aim at the restoration of sight by transplanting a transparent cornea, we must content ourselves with a small corneal graft, removed along with conjunctival bands, which not only assist to keep it in position, but also by their adhesion afford it a chance of preserving its vitality, even if we do not get immediate union between the corneal edges. (Dr. J. R. Wolfe, p. 256.)

FOREIGN BODY IN THE EAR.—*Removal of.*—When syringing has failed, take a rather large and broad curved needle, and rub the pointed end over a hard metal surface (as the flat of a knife) so as to burr it over towards the concavity of the needle. A fine yet highly resisting hook is in this way produced, affording a means of powerful traction on the foreign body when the needle is passed between it and the wall of the meatus. (Mr. A. G. Brown, Lancet, Dec. 27, p. 940.)

GLAUCOMA.—*Sclerotomy.*—I recommend sclerotomy in the following conditions. 1. In the incipient stage of glaucoma when, between the attacks, vision is normal. There is no doubt but that the formation of an artificial pupil in such eyes, even when completely successful, gives rise to an imperfect optical apparatus, causes dimness, and frequently astigmatism; and the patient, comparing the vision left him after the operation and that which he previously enjoyed, is always more or less discontented, although he may possess a central acuity of vision which is perfect. 2. In cases of simple glaucoma, in which both central and peripheral vision are nearly normal. The same considerations as to the deteriorating action of iridectomy are equally applicable here. 3. I recommend recourse to sclerotomy when, in simple glaucoma, the margin of the visual field, even at one side only,

touches slightly or comes very near to the point of fixation. Every clinicist is aware that in such cases the iridectomy is very frequently followed by a further diminution in the field of vision, which may deprive the patient of central vision, a danger not to be feared with sclerotomy. In all the sclerotomies which I have lately performed, I have used a narrow Graefe's knife, which I found more suitable in cases of shallow anterior chamber than the sclerotome which I formerly recommended. Moreover, as also Mauthner has recommended, I do not confine myself strictly to the formation of a flap two *millimètres* high, of which the middle third is left undivided. But, especially if the depth of the anterior chamber permit it, I form a flap of from three to four *millimètres* high. The knife should penetrate the sclerotic very precisely at a distance of one *millimètre* from the clear cornea, and then, the blade being held perfectly parallel to the plane of the iris, it should be passed very slowly in front of the latter, so that the counterpuncture may fall also exactly at a distance of one *millimètre* from the internal border of the cornea. If the blade be not held quite parallel with the plane of the iris, there is danger that it may pass through the cornea, and that the sclerotomy may be but half performed; or, if it be directed too deeply, it may come out through the sclerotic at an exaggerated distance from the cornea, may wound the ciliary body, and may provoke troublesome hemorrhage and irritative symptoms of an alarming kind. A complete curative effect should not be expected from sclerotomy unless it is performed strictly *lege artis;* and, in order that this may be, it is necessary that the operator should have the proceeding at his fingers' ends. I would like every practitioner to be convinced that it is only after a certain apprenticeship that good and precise sclerotomies can be performed. The section should be made by slow sawing movements, leaving exactly one-third of the flap undivided. (Dr. L. de Wecker, p. 267.)

OTITIS.—In an old standing case of otitis media, accompanied by destruction of the membrani tympani, large granulations, and profuse discharge, ergotin was applied directly with a camel's hair pencil, after having been diluted with sufficient glycerin to make it flow easily; the result was satisfactory, the granulations having shrunk rapidly, while the discharge disappeared and the sensitiveness abated. (Dr. S. Eldridge, New York Medical Journal, Oct. 1879.)

TOBACCO AMBLYOPIA.—The existence and frequency of tobacco amblyopia is well established. The sight in tobacco amblyopia is nearly always best in a rather dull light, the com-

plaint being very much the same as in early uncomplicated nuclear cataract. The patients, when asked whether the sight is always bad alike, generally say that it is better the first thing in the morning and again towards evening, that it is better on dull days, and that in direct sunlight it is very bad. It is no doubt, as in nuclear cataract, related to the size of the pupils, sight being rather better when they are slightly dilated. It does not depend upon any periodical changes in the state of the nerve or retina. (Mr. E. Nettleship, p. 275.)

MIDWIFERY, ETC.

CERVICAL METRITIS.—In cases of cervical metritis, ergot and belladonna may be combined in the following proportions to form pessaries:—Ergotin (or solid extract of ergot), gr. xx.; extract of belladonna, gr. ii.; cocoa butter, q. s., mix and make into six pessaries; one to be inserted into the vagina every night, after using the hot douche. In warm weather these remedies may be dissolved in glycerine and water, as in this formula:—Ergotin, gr. iii.; extract of belladonna, gr. vi.; water and glycerine, āā ʒiv.; mix. A pledget of cotton is to be saturated with this solution, and inserted into the vagina at bed-time, after the hot douche; the cotton should be removed in the morning. (Dr. W. C. Dabney, American Journal of Medical Science, July 1879.)

CERVIX UTERI.—*Dilatation of by Elastic Vulcanite Plugs.*—Mr. Tait, of Birmingham, uses vulcanite plugs most effectually. The advantages of the plan are that it is absolutely free from smell and septic risks, in these matters contrasting most favourably with the use of sponge tents. It is also almost free from pain, and here it has a most incontestable advantage over the use of sea-tangle tents. It is likewise superior to either of these methods of dilating the cervix in that the plugs, being of vulcanite, can be used indefinitely, and that, therefore, their use is far less costly. I may also say that I have never obtained from sponge, tangle, or tupelo tent the complete dilatation of the whole canal which these plugs produce. The dilators are best arranged in a set of four sizes made to screw on to a common stem. They are sold to any pattern by Messrs. Mappin and Co., New Street, Birmingham. The only precaution necessary in their employment is to use extremely gentle pressure. I have had a bent stem made for exceptional cases of flexion, but in the great majority of instances the straight stem answers every purpose. (Mr. Lawson Tait, p. 307.)

ERGOT IN LABOUR.—I do not believe in spurring a jaded horse; and, therefore, very seldom give ergot, except in cases where I have reason to expect *post partum* hemorrhage. Then, just before delivery is complete, a dose of ergot injected deep into the gluteal region very rarely fails to secure perfect uterine contraction. (Dr. Jas. Hickinbotham, British Medical Journal, Sept. 20, p. 450.)

IMMEDIATE PLACENTAL DELIVERY IN NATURAL LABOUR.—Dr. S. Elder, of Indianapolis puts forth, in the *Transactions of the Indiana Medical Society*, the theory that delivery of the placenta should be as nearly coincident with the delivery of the child as may be; and this result he works for by kneading the fundus of the uterus through the abdominal walls as the child passes the outlet, endeavouring thereby to induce the expulsion of the secundines by the same pains that effect the delivery of the child. If this measure be not sufficient, he immediately, before any attention to the new born, gives manual assistance *per vaginam*, insisting that, where such manipulation is called for, it should be done before the maternal organs have recovered from the shock caused by the passage of the child, for the dual reason that it is less perturbating to the mother and more facile for the accoucheur. Illustrating his theory by the detail of seven cases, Dr. Elder says that his process affords security against *post partum* hemorrhage, against hour-glass contraction, against uterine inertia, against adherent and retained placenta, against frequent severe protracted after-pains, and against septicæmia and prolonged lochial discharge; that it saves valuable time, and saves suffering and anxiety on the part of the mother; that it is better for the child, as the cord is not tied until the placenta is delivered; and that it is in accordance with the natural indications, and carries out the physiological indications of the process of complete parturition. (British Medical Journal, Jan. 3, p. 20.)

IMPENDING DEATH FROM UTERINE HEMORRHAGE.—*Nitrite of Amyl.*—In threatened death from uterine hemorrhage heat applied to the head prevents anæmia of the brain and so may save the life of the patient. The same thing may be more rapidly and effectively done by means of nitrite of amyl. When attending a case of rather severe *post partum* hemorrhage, and, happening to have some of the drug with me, I at once put my theory in practice by administering five minims by means of a Skinner's inhaler, and, I am happy to say, with immediate and most satisfactory results. The hemorrhage ceased at once and permanently, and the patient was restored from a state of collapse. Indeed, in this latter

respect, its effect was only comparable to that of the hypodermic injection of ether. (Mr. Elias W. Kerr, p. 334.)

INTRA-UTERINE MEDICATION.—Various procedures have been employed for applying caustic or alterative medicaments to the interior of the uterus, either in the form of ointments, or of solid substances which melt in the uterine cavity, and spread themselves over its lining membrane. Many instruments have been invented for this purpose, by some of which a piece of solid nitrate of silver or some other substance is pushed into the uterine cavity and left there; by others, a medicated plasma or ointment is similarly introduced. Sticks or crayons of various substances, such as sulphate of zinc, alum, and the like, are by some inserted into the cervical canal, and allowed to melt. I have made many experiments with these plans, and have long discarded them, believing them to be far more troublesome to use, more painful to the patient, and less efficacious than the method of applying the chosen medicament to the uterine cavity in a fluid form, and swabbing its mucous membrane freely with it, which I shall now briefly describe. For this purpose, I use small probes of flexible metal, roughened with emery paper, two inches and a half in length, and attached to a wooden handle. Before operating, three or four of these are covered with a thin film of cotton wool. A small piece of cotton is flattened between the palms of the hands; the probe is dipped in water to make the cotton adhere, and, by a little practice, the cotton is wrapped round it so firmly that it cannot be stripped off by any ordinary force, and so smoothly and evenly as scarcely to add to the bulk of the probe. The patient is placed on her left side, and the cervix exposed by a full-sized Ferguson's glass-speculum, which answers better, I think, than either the duck-billed or glass-speculum, as it embraces and steadies the cervix. Before commencing, it is advisable to pass the sound at least once, to ascertain the direction of the uterine cavity, and the probes must be bent to accommodate themselves to it. With one or two of the mounted probes, the cervical and uterine cavities are thoroughly swabbed out, so as to clear them as much as possible of the discharges which might otherwise prevent the medicaments used from reaching the mucous membrane efficiently. If the discharge be very tenacious and adherent, it may be advisable to moisten the cotton with water or glycerine to facilitate its removal. Another probe is then dipped into the solution to be used, and the cavity is thoroughly swabbed with it. In the majority of cases of endometritis, there is no difficulty whatever in passing the

probes, since the cervical canal is abnormally patulous, and they enter with the greatest ease. A great variety of substances have been employed. Amongst these may be mentioned nitrate of silver in solution, tincture of iodine, chromic acid, carbolic acid, solutions of the sulphate of zinc or copper, the acid nitrate of mercury, and fuming nitric acid. After many experiments, I have arrived at the conclusion that the safest and most reliable agents are tincture of iodine, and a mixture of equal parts of crystalline carbolic acid and glycerine, the latter being that which I generally use. I conceive that what we require in most cases is not a solution which will cauterise and, if sufficiently strong, destroy the tissues to which it is applied, but one which will so modify the circulation as to cause a healthy reparative action in the diseased structures. (Dr. W. S. Playfair, p. 318.)

It is now admitted, as an essential preliminary of intra-uterine medication, that it is indispensable to establish a free passage through the cervix for all fluids injected into the womb or by it secreted; and I have found the dilatation of the whole cervical canal by laminaria-bougies, a day or two before menstruation, to diminish greatly the pain and blood-loss attendant on membranous dysmenorrhœa and the uterine irritation that follow the expulsion of the membrane. With regard to treatment, when internal metritis is leading to death by blood-loss, I recommend the injection of the undiluted tincture of iodine; for, by doing so, I have repeatedly checked flooding, and also uterine exfoliation in membranous dysmenorrhœa. Tincture of iodine commends itself by antiseptic as well as by hæmostatic properties. (Dr. E. J. Tilt, p. 323.)

With respect to uterine medication, the results are likely to be serious. If an application to the cavity of the body of the womb be required, there should be an amount of *endometritis* calling for it; but, in how many cases of discharges from the os uteri does the exudation come from above the canal of the cervix—the so-called internal os? In very few. The probe, covered with cotton wool saturated with carbolic acid, is passed along the cervical canal up to the fundus uteri, and in many cases good results are obtained—not because the fundus uteri has been reached, but *because the application has been made to the canal of the cervix;* and, had a zinc-alum point been placed in this situation, and the uterine cavity have not been reached, the result would have been equally satisfactory, without the risk of danger possible to occur from probing the cavity of the womb. (Dr. Godson, p. 329.)

Iodised Phenol.—For intra-uterine medication Dr. Battey uses iodised phenol. He says, the standard solution employed consists of one part by weight of iodine dissolved in four parts of liquefied carbolic acid, and to this solution I have given the name iodised phenol. Iodised phenol is believed to be simply a concentrated solution of iodine in carbolic acid, and not in a proper sense a chemical compound. It is black in colour, syrupy in consistence, and possesses in marked degree the pungent odour of iodine, which is rapidly given off when it is heated. Since its introduction into my practice, the iodised phenol for intra-uterine medication has been employed by me to the almost entire exclusion of other remedies. The instrument employed in making the application may be any probe or sound which will easily enter the canal. I specially prefer to use a rather slender and elastic hard india-rubber probe, made slightly tapering, and with a blunt, not bulbous, point. The elasticity of this probe allows it to yield rapidly to pressure, to change its course, to follow easily the canal of the cervix, and to enter the uterine cavity proper, and this in spite even of a moderate flexion or version of the uterus. From the cotton-factory is obtained cotton wool in the form of an untwisted rope or roll, the fibres of the cotton being perfectly straight, and lying parallel to each other. It is technically known to the cotton-spinners as "the lap," and can be purchased of the best quality at our factories for eightpence to tenpence sterling per pound. It is admirably suited for general gynecological uses. Having selected six or eight of the elastic probes, I break off from the cotton "lap" four or five inches, and with my fingers separate or split it into several fasciculi of such size as, when wound upon the probes, will enlarge them to the thickness desired. The end of a probe is now moistened slightly, and the fasciculus of cotton wound spirally upon it. The cotton-armed probe is now dipped into the iodised phenol, any redundancy is allowed to drip away, and the probe is passed into the uterus with a slow spiral movement as it advances. At first the probe is introduced but a short distance, and immediately withdrawn, and the case rests here to test the tolerance of the uterus for the remedy. At subsequent stages, the probe may be carried to the fundus, and followed immediately by a second, and even by a third or fourth, if well borne. The remainder of the wrapped probes are employed for wiping off the cervix or vaginal wall any of the phenol that may have touched those tissues. The energy of the application is regulated by the size of the wrapping, the depth to which the probe is passed, and the number of medicated probes used. When a very decided impression is to be made, a backward turn is given

to the probe in its withdrawal, so as to leave the saturated cotton in the uterus, there to remain twenty-four hours, or even until it is spontaneously expelled. The application is renewed every four to fourteen days, according to the energy of the treatment. I have abandoned the use of sponge-tents in connection with the treatment set forth. When dilatation is required, the cotton-wrapped probe is employed, and the cotton left as a soft tent in the canal. The dilating power of this is notably less than of sponge, but nearly equal to sea-tangle, and, it is believed, entirely safe. (Dr. R. Battey, p. 326.)

New Instrument for Intra-Uterine Medication.—In the introduction of the probe armed with cotton wool soaked in carbolic acid or other fluid, much of that fluid is wiped off before the instrument reaches the part upon which it is desired to act. By the use of the following instrument this is avoided. A silver canula, eleven inches long, of the calibre of a No. 6 catheter, bent at an obtuse angle three inches from its extremity, and having a knob on its convexity two inches and three-quarters from its extremity like that on an uterine sound. It has a stilette two inches and a half longer, the projecting portion slightly roughened, terminating in a round knob, which, when the stilette is drawn in, closes the end of the canula, facilitating its introduction, and confining the fluid with which the cotton wool on the stilette is soaked. The stilette has also a sort of wedge-shaped collar, two inches and a half from its extremity, to enable it to be drawn back into the canula before the instrument is withdrawn from the uterus. The wool should not be wrapped on the stilette too tightly; and in drawing the stilette with the soaked wool into the canula, it should be guided in with the fingers, so as not to allow wool and fluid to be scraped off on the end of the canula. The button-extremity of the stilette now completely closing the end of the canula, the instrument can be passed up to the uterine cavity without the fluid coming into contact with any part of the passages. After reaching the fundus, the canula is retracted on the stilette (not the latter pushed forward), and the fluid then comes into free contact with the mucous membrane. (Dr. J. W. Miller, p. 328.)

INTRA-UTERINE TUMOURS.—How are you to make sure of the presence of an intra-uterine tumour? First, you are not to attempt to make sure unless you have sufficient reason; for the process of making sure is itself attended with considerable danger—the danger of septicæmia from the injuries the process may cause; the danger of parametritis or perime-

tritis; which must always enter into our consideration. Let us suppose, however, that the case is serious enough to demand that you proceed. You must get your finger into the inside of the woman's uterus to feel it. Examination by the probe is often spoken of, but it is utterly unsatisfactory: there is only one sort of probing that is conclusive for this kind of diagnosis, and that is with the living, educated finger; the other hand aiding by acting in the bimanual method. This is especially successful in cases where the cavity of the body of the uterus is dilated: then you may be able to insert your finger without further ado, without previous artificial dilatation; or you may, by the exercise of a little force, push the finger through the external and internal orifices; or, again, you may succeed by pressure, while the neck of the womb is held in a vulsella, to prevent its receding before your finger, or to pull it down on your finger. The best method of dilatation is by means of tangle tents. Sponge tents are often used; I prefer the tangle, meantime at least. You must have a tangle tent at least three inches long. A uterus which is much hypertrophied may require even a longer tent than one of three inches to open it thoroughly. Let us suppose that you have completed the dilatation: you have next to introduce your finger into the cavity so as to touch the fundus, and for this purpose you will probably require to hold or pull down the cervix with a vulsella, upon your finger, in the same way as you pull a glove on a finger. As to treatment, I recommend you to trust in *avulsion*. Do not first separate the tumour and then take it off, but use avulsion, doing the two parts of the operation simultaneously. In the great majority of cases nothing else is required. You seize the little tumour with a vulsella, and with a slight amount of rotation pull it out: it is, if a fibroid, enucleated by the violence. Of course, if it is a fibroid and already partially enucleated, it comes away with no difficulty; but even if it is covered by a thin capsule, by seizing it you can get it away without much trouble. If you should require any cutting, I recommend you to use a pair of curved scissors, though this is very seldom necessary in the case of a fibroid. In the case of a soft mucous tumour which is not a polypus the process of removal resolves itself, involuntarily on your part, into one of torsion and pulling away. You seize the tumour with a pair of uterine dressing forceps, and pull it off just as you would pull off a nasal polypus. In both sets of cases the process is essentially one of avulsion. In the case of adherent placental masses you peel off with your nail or with the tip of your finger. (Dr. J. Mathews Duncan, p. 311.)

IRREGULAR MENSTRUATION.—*Picrotoxine.*—Dr. Phillips recommends picrotoxine for certain symptoms associated with irregular menstruation. In females of nervous temperament and of thin and delicate fabric of body, the menses are often preceded by paroxysms of colic felt in the hypogastric region, and accompanied by more or less pain in the back and hips. This pain not only precedes the appearance of the catamenia at each period, but accompanies them for the first day or two. They are of a twisting, griping, or colicky character, and are attended by a scanty discharge or a profuse one, in either case somewhat paler than usual, and mixed with clots and shreds of membrane. The administration of two or three drops of a saturated tincture of cocculus three or four times a day prior to the expected flow, and continued during the first two or three days of its progress, will frequently ward off the pains and render the discharge more natural. (p. 97.)

MAMMARY EPITHELIAL TUMOURS.—It is now many years since Mr. Birkett called attention to the existence in the mamma of epithelial growths, which are distinguishable from scirrhous cancer of the organ, and pointed out clearly their salient features. The concurrent testimony of the most experienced modern observers goes to confirm the doctrine that there are two distinct kinds of epithelial growth in the mamma. In one form, frequently recognised as scirrhus, the growth tends to diffuse itself rapidly through the connective tissue of the breast, and absorption by the lymphatics is the rule. In the other form, the epithelial growth is found in circular groups or clusters, surrounded by connective tissue, into which it has usually a comparatively slight tendency to penetrate; and around the circular cell-groups a distinct limiting membrana propria is often found—a fact to which German observers are prone to attach much importance. Dr. Thin, however, states that the membrana propria is not invariably present, and regards it as a new formation, frequently but not invariably found around the cell-masses. In scirrhous cancer the membrana propria seems never to be found. (Editor of British Medical Journal, p. 330.)

PLACENTA PRÆVIA.—We ought to distrust the old practice of the *accouchement forcé*. It is unscientific, violent—one might say brutal—and dangerous. We must adopt methods more rational and more safe. We must give time for the cervix to expand; remove the obstacle to its expansion which the adhesion of the placenta over the os internum presents by detaching it from this region, expand it gently by water-pressure if need be, not deliver until the parts are fairly dilated; and, if turning be indicated, to accomplish this by

the bipolar method, which renders the introduction of the hand unnecessary. (Dr. R. Barnes, p. 309.)

PUERPERAL FEVER.—In the treatment of puerperal fever, boracic acid, combined with sulphuric ether (which is also an antiseptic), and when it has been found necessary a little tincture of opium has given decidedly beneficial results. (Dr. F. P. Atkinson, Practitioner, April, p. 254.)

SORE NIPPLES.—Chapped nipples are treated either by the glycerole of the nitrate of lead in a solution containing from twenty grains to one drachm to the ounce, or by a mixture of two drachms of iodoform to the ounce of balsam of Peru. The balsam is used because it disguises the smell of the iodoform. (New York Medical Record, Nov. 29, 1879.)

UTERINE DOUCHE.—*Mr. Masterman's New.*—It is often a matter of great importance to be able to thoroughly wash out the vagina or uterus whilst the patient is lying in bed; but to her the performance is usually a very disagreeable one, even when done by the medical attendant himself, from the difficulty in preventing the escaping fluid from wetting the bed, and from the almost unavoidable exposure of her person in adjusting or removing the receptacle employed; and, if the operation be delegated to the hands of an untrained nurse, the patient generally complains of the pain inflicted in addition. I have had a douche made for me by Messrs. Maw, Son, and Thompson, which completely removes these difficulties, and is, at the same time, so simple that it can be used successfully by the most unskilful of nurses. It is an India-rubber tube, ten inches long, 1.25 inches in diameter, and 0.3 inch thick, terminated by a thin polished cone of vulcanite, and accompanied by a slender injecting tube, two inches longer, which fits over the jet of a Higginson's syringe. After being dipped in warm soap and water, it can be passed with the utmost facility whilst the patient is lying on her left side in the ordinary obstetrical position, even when the labia are swollen and tender; and it is long enough to reach a basin placed outside the bedclothes. The injecting-tube can then be painlessly introduced as far as may be necessary, and a second basin being placed beside the other filled with warm water, Condy's fluid, &c., the washing away of clots, fœtid discharges, and so on, can be carried out; or, if required, a lotion may be retained as long as we please by raising the end of the larger tube; and in both cases, without a drop of the fluid escaping between it and the labia. The only precaution to be taken is to press the edge of the basin close to the nates when finally withdrawing the tube. (Mr. Masterman, British Medical Journal, March 20, p. 445.)

MISCELLANEA.

ANÆSTHETIC.—*Ethyl Bromide, or Hydrobromic Ether.*—This agent, the latest claimant of public favour as an anæsthetic, was first introduced into professional notice by Dr. Turnbull of Philadelphia, and seems to be gaining some reputation among the surgeons of that city. Dr. G. F. Gowers gives some short notes of fifteen cases operated on under its influence, and, judging from these, its general effects may be stated as follows:—As a rule, moderate acceleration of pulse and respiration; slight excitement or talking, seldom any struggling; flushing of face; dilatation, sometimes preceded by contraction of pupil; diaphoresis, occasionally profuse; complete anæsthesia in two to three minutes; recovery of consciousness in *one to two* minutes after withdrawal of anæsthetic; no after vomiting. The quantity used in these fifteen cases varied from 3 to 11 drachms; in the latter case, the operation, amputation of thigh, lasted forty minutes. In no instance, as yet, have dangerous symptoms presented themselves. The advantages claimed for ethyl bromide are: 1. Safety, so far as any anæsthetic is safe. 2. Promptitude of action; in this respect it excels ether. 3. Smallness of quantity required to produce anæsthesia; two drachms will, in one to three minutes, produce anæsthesia as profound as that produced by an ounce of ether. 4. Slightness of action on respiration and circulation. 5. Rapidity with which consciousness returns when anæsthetic is withdrawn. 6. Absence of depression or inclination to vomit after its use. 7. Pleasantness of odour. 8. Non-inflammability. Dr. Gowers gives the following directions for its administration:—"A small napkin should be folded and pinned to a folded towel that covers the patient's face; on the napkin two *measured* drachms of the bromide should be poured, and the patient directed to take long, deep inspirations, or what is better, to make prolonged and forced expirations; in two minutes from the time of administration of the first two drachms, a second drachm should be given, and this should be repeated at intervals of two minutes." (Glasgow Medical Journal, March, p. 259.)

BORACIC ACID.—Considering the well-known antiseptic properties of boracic acid, it is exceedingly curious how little it has been administered as an internal remedy. It ought to hold an important place in the treatment of carbuncular disease—erysipelas, cholera, scarlatina, enteric, typhus, and intermittent fever—and in fact all those cases which are known to have a septic origin. It is but sparingly soluble in cold water; an ounce will only take up about 18 grains, but a drachm

of boiling water will dissolve about 5 grains. The dose is from 5 to 15 grains. It has one particular recommendation, and that is its tastelessness. (Dr. F. P. Atkinson, Practitioner, April, p. 254.)

CHLORAL.—Some authors have asserted that chloral is not soluble in fatty bodies, but in the Rep. de Pharm. M. Catillon shows that this statement is erroneous, and he proposes various formulæ which it may be useful to reproduce. Chloral dissolves in oil in all proportions under the influence of a slight elevation of temperature, and in cold three parts of oil dissolve two parts of hydrate of chloral.

Chloral Liniment.—Hydrate of chloral, 6 parts; oil of sweet almonds, 30 parts. Rub together in a mortar, and assist the solution by gently heating the mixture over a water bath.

Chloral Ointment.—Hydrate of chloral, 6 parts; lard, 27 parts; white wax, 3 parts. Melt together, having previously powdered the chloral. The wax is not indispensable, and may be omitted unless a very firm ointment is desired. Equal parts of wax and chloral yield a mass of the consistence of plaster. Suppositories may also be prepared with white wax and cacao butter, containing one part of chloral in five, or even one part in three. (Dublin Journal of Medical Science, Dec., 1879.)

DISINFECTANTS.—As the result of a great number of experiments it seems clearly proved that deodorisation and disinfection are quite different things. Mere deodorants do not by any means necessarily destroy morbific matter. As the *chloride of lime* has taken first place in these experiments I would observe that this agent would be more effective than it is commonly found to be if it were previously mixed with water, and then used when well shaken up, after the manner of other liquid disinfectants, instead of being rudely thrown about in the dry state, or mixed up with a little water in a saucer. In these experiments is was found that a solution containing 140 grains of chloride of lime to the gallon was strong enough to disinfect a fluid such as that used in the experiments detailed. (Dr. J. L. Notter, p. 45.)

DISINFECTION BY ACIDS.—*Hydrochloric Acid.*—This acid, consisting of one of the strong acid to twenty of water, is used chiefly to disinfect typhoid excreta, and the bed and body clothes of persons with infectious disease. In typhoid cases, a large cupful of this fluid is put in the vessel before it is used. After being emptied down the watercloset, it is rinsed with another cupful, which is also at once put down the closet, the water running while this is being done. In using this fluid

for clothes, these only require to be sprinkled with it, so as to sensibly damp them, allowing them to remain so for about four hours; but of course saturation for about an hour is preferable. With thick woollen clothes, such as blankets, a large quantity of the acid fluid is needed for saturation. Wool resists the action of acids far more than cotton or linen, and as this material is more likely, from its porous nature, to retain infection than the latter, it should be subjected longer to the action of the acid. Boiling water is afterwards poured over the acidified clothes, which are allowed to stand for an hour. They are then thoroughly rinsed with warm water till all the acid is got rid of, when soap with soda or potash may be used to wash them. If the acid be not removed, the soap will not form a lather. The solution of this acid may also be used for sprinkling on floors, pouring down sinks, &c.

Sulphurous Acid.—This is used exclusively for aërial disinfection, which is done morning and night during the patient's illness, not only in the patient's apartment, but in every room in the house. The process is simple; a piece of live coal is placed on a shovel and powdered sulphur sprinkled on it. The sulphurous fumes thus generated should be made to impregnate the entire atmosphere of the house, until it becomes somewhat irritable. If more fumes have been generated than can be tolerated, the excess is easily got rid of by opening the doors and windows, and by allowing what may be still forming to ascend the chimney. Fifteen minutes is sufficient to allow the acid to act each time it is used; but if the apartment be empty, it may be used some hours. Mattresses and carpets can be disinfected with sulphur fumes by suspending them spread out by the four corners with ropes, and allowing the acid to float thickly along the lower surface for ten minutes, then turning them upside down, and repeating the process. Bright metallic surfaces are darkened by sulphurous acid.

Aromatic Glacial Acetic Acid.—Though more than a hundred times the price of the others, this acid gives great satisfaction in the using, its well known grateful cooling odour forming a pleasant contrast to the repulsive emanations from the body of an infectious patient. It consists, besides the acid, of the oils of rosemary, neroli, cinnamon, cloves, bergamot, lavender, and rectified spirit. I frequently use this acid as an occasional aërial disinfectant, but never to the exclusion of sulphurous fumes. It is directed to be dropped on a hot shovel and volatilised through the house daily, which is the more effective way; or, where its price is a consideration, a little may be poured occasionally into a small vessel of hot

water, placed over the gas, in the patient's room, by which means it becomes more gradually dissipated, producing at the same time a constant pleasant odour in the apartment. I have had much satisfaction also in using it dilute (one drachm to one ounce of water) for sponging the body, by which two objects are attained—lowering of the temperature and disinfection of the skin. Almost without exception patients like this process, and are usually much refreshed by it. In all scarlatinal cases, more especially during desquamation, I invariably direct this to be done twice a day. The practice, so much in vogue, of smearing the skin of a scarlatinal patient during desquamation with lard or warm suet, to prevent the morbific epithelial *débris* from scattering, seems to me highly calculated to injure the sufferer. By this means the mouths of the sebaceous and sudoriparous glands, the very channels through which, in all probability, the poison finds egress from the blood, are plastered up; so that, besides the unpleasantness it causes, it is an effectual way of sealing the infective matter in the body. It looks precisely analogous to hermetically closing every door, window, crack, and crevice in a room where a person is lying soaked with infection. On the other hand, by sponging the skin with the aromatic acid, the infectious scurf is permanently deprived of its dangerous properties, while, at the same time, the numerous orifices of the gland ducts are cleared of their morbid secretions, thus giving free exit to the poison from the blood. Moreover, it is well known, and has been proved experimentally, that the evaporation and oxidation of most perfume-yielding oils, including those in aromatic acetic acid, generate ozone—a powerful disinfectant. Here, then, is another strong recommendation for sponging the body with the substance in question, as by this means an atmosphere of ozone may actually be made to evolve gradually and constantly from the patient's skin, floating under the bedclothes and enveloping every part of the body. (Dr. J. Dougall, p. 48.)

SALICYLATE OF QUININE.—Salicin, salicylic acid, and salicylate of soda have obtained a reputation in the treatment of rheumatism. But no notice has yet been taken of another salicylate, which seems likely to secure a high place in the treatment of that malady—I mean salicylate of quinine. The first mention I had of it was made to me in a letter by a patient resident with her family in Paris during the past winter. One of her sons had a sharp attack of rheumatic gout. Salicylate of quinine was prescribed for him, and she wrote of the effects as wonderfully rapid and satisfactory.

Ten days ago she returned to London and sent for me to attend one of her family, who had suffered severely in their rough passage across the channel. For three days previously I had been suffering from rheumatism in the left knee and thigh. It had steadily increased to such an extent that I could hardly walk at all; and when I sat down it cost me much pain and difficulty to rise up, and still more to make any progress. My movements were awkward and distressing to myself and to lookers-on. My good friend was very sympathetic. In her usual kind and practical way she ordered me "home to bed," and early next day, as soon as she could get to her boxes, she sent me a note with a bottle of the salicylate of quinine, which she had brought over with her. Pressing engagements prevented me remaining in bed that forenoon. But by four o'clock in the afternoon I was dead beat, and quite incapable of further work. Between four and five I retired to bed. For two days previously I had fever on me, which had gradually increased, and now my temperature stood at 101°. My usual temperature is 97·5° to 98°. I at once took a dose of salicylate of quinine—twenty centigrammes—as directed on the bottle. In bed I could not move my leg or turn myself without the greatest difficulty and suffering. During the night I perspired freely. Next morning, Feb. 22nd, there were still fever and pain, but the temperature was considerably less. All that day I remained in bed, and I took three doses of salicylate of quinine at intervals of four hours. In the evening and during the night I felt I could move myself altogether with greater ease, and I had a quiet good night. Next morning, Feb. 23rd, I felt completely relieved. To my astonishment (for this was not my first attack), I got out of bed and walked across the bedroom without the slightest limp. The posterior muscles of my thigh certainly felt sore as if they had been severely strained and bruised, but I felt as able for my work that day as if nothing had happened, and I did a long day's work. It is necessary not to discontinue the medicine too suddenly. Salicylate of quinine, though not usually employed hitherto in London, is to be procured. The difficulty I have found is to get it easily dissolved, and kept in solution. Mons. L. Midy, a pharmacien of Paris, 113, Faubourg St. Honoré, has made an elegant preparation of this drug. It is one I have used, and I can recommend it. But I have no doubt our expert London chemists will be able also to master any difficulty that may appear at first sight to exist. (Dr. A. Hewan, London, Lancet, April 3, p. 540.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART 1.—ON CONTAGION: ITS NATURE AND MODE OF ACTION.

By JOHN SIMON, Esq., C.B., F.R.S.

The word "contagion" is applied in pathology to the property and process by which, in certain sorts of disease, the affected body or part causes a disease like its own to arise in other bodies or other parts; and the Latin word *contagium* (pl. *contagia*) is conveniently used to denote in each such case the specific material, shown or presumed, in which the infective power ultimately resides.

The property of contagiousness belongs to a very large number of the diseases which affect the human body. And in more than this direct way the property is of great interest to mankind. Contagiousness of disease is a fact not only for man, but apparently for all living nature; and the influence of contagion in spreading destructive diseases among domestic animals, and among those parts of the vegetable kingdom which contribute to the nourishment of man, is such as to make it of immense social importance that the laws of contagion should be well studied and understood. Further, just as contagion in the case of living bodies and their parts spreads disease from one to another, so, to an immense extent, in the case of certain matters which, though of organic origin, are not living, it spreads various processes of decay. The so-called fermentations which yield alcohol and vinegar, as well as that in which putrefaction consists, are contagious affections of the respective matters in which they occur: every cheesemonger knows that moulds of different kinds spread by inoculation, each in its own kind, from cheese to cheese; and if the Greek proverb "grape mellows to grape" be true of the living fruit, the apple-loft gives analogous experiences of contagion among the fruit which is garnered.

The rationale of the word "contagion" as now used, is that the property is understood to attach itself essentially to a material contact; not necessarily that, when infection is spread from individual to individual, the contact of the individuals must have been immediate; but that in all cases there must have been such passage of material from the one to the other

as was in itself at least a mediate contact between them. And similarly, in those very instructive illustrations of the process of contagion which are furnished within the limits of a single diseased body by the propagations of disease from part to part of it, we can in general easily see that infection advances from part to part, either in proportion as part touches part, or in proportion as one receives from the other the outflow of lymph or blood or secretion.

The various specific matters which effect contagion in the living body, the respective "contagia" of the given diseases, seem all to have in common this one characteristic: that in appropriate media (among which evidently must be counted any living bodily texture or fluid which they can infect) they show themselves capable of self-multiplication; and it is in virtue of this property that, although at the moment of their entering the body they in general do not attract notice, either as objects of sense or as causes of bodily change, they gradually get to be recognisable in both of these respects. Now, the faculty of self-multiplication is eminently one of the characters which we call vital; and when it is said that all contagia are self-multiplying things, this is at least very strongly to suggest that perhaps all contagia are things endowed with life.

In order to any general consideration of the question thus suggested, contagia may conveniently (even if but provisionally) be distinguished as of two main classes, differing, or at present seeming to differ, from each other in their mode of action on the organisms which they infect: one class, namely, that of Parasites; and the other class, that of the true or Metabolic Contagia. Of this separation, so far as present knowledge seems to justify it, the assumed grounds are: that each true contagium, in proportion as it multiplies in the body, transforms, in a way which is specific to itself and is different from the ways of other contagia, the bodily material with which it has contact; while, on the contrary, the parasite, however much it may grow or multiply in the body, produces no qualitative effects specific to itself, but only such effects as are of common kind to it and all other parasites—indications, namely, of its mechanical intrusiveness in the parts which it occupies, and sometimes of the drain which it makes on its host's general nutritive resources.

A.—Of Parasites, in relation to processes of contagion little needs be said in the present article. When an organism or part of it is, in greater or less amount, inhabited by other organisms, animal or vegetable, which subsist on it, or its food or refuse, it of course may be a centre of infection to other (if susceptible) bodies or parts, to which it can transmit live parasites or their germs or seeds; for, when this transmission takes

place, growth and self-multiplication, as in a colony, are the natural results which have to be expected; and in proportion as these occur, the newly-infected body or part gradually gets to suffer, like the old, from these particular derangements which make the type of parasitic disease. Some parasitic diseases, especially some of those of the skin, spread actively by direct contagion in ordinary intercourse; as, for instance, scabies by the migration of its acari, and porrigo (among children) by the spores of its microphyte; and the spreading of such diseases where they exist may, of course, be to any extent facilitated by aggregation of persons and uncleanliness of personal habits. There are cases in which parasitic diseases spread from animal to animal only in proportion as the one feeds on the other, and eats it with parasites still living in it; or in proportion as live parasite eggs or larvæ, discharged from the body of one animal, get conveyed with food (especially on raw herbage and in water) into the bowels of another. Considerable epidemics of trichiniasis in the human subject have been traced, chiefly in Germany, to infection from the pig, in cases where pork, abounding with trichinæ has been eaten, as sausage-meat or otherwise, in a raw or imperfectly cooked state; and in Iceland the very great sufferings of the human subject and the cattle from echinococcus have been traced to the influence of the dogs in spreading contagion from the slaughter-house, where they eat hydatidised offal, to the kitchen-gardens and water-sources and pastures, where they discharge tapeworm eggs from their bowels.

The diseased states which consist in being colonised by parasites are diseases of indefinite duration, tending in some cases to indefinite increase. In cases where the disease consists in the presence of swarms of blood-sucking or otherwise exhausting animal parasites, symptoms of the blood-drain will of course gradually arise; but otherwise the parasite, whether animal or vegetable, operates only as a mechanical presence. Skin and mucous membrane will be irritated in their superficial layers, and in some cases more deeply, by the animal or vegetable parasites which breed on or in them, just as they might be by dead mechanical irritants: solid organs having cystic entozoa in them will in like manner show evidence of irritation by encapsulating the colonists; and the surrounding tissue will of course suffer compression and displacement in proportion as the colonisation (*e.g.*, in case of echinococcus) is compact and massive. In the case of trichiniasis—but, in our ordinary experience, in no other—the multiplication of the parasites, the burrowing of their young, and the general diffusion of these in the body, are processes of such extreme activity that, if the quantity of contagium taken into the stomach have been large, the innumerable local irritations suffice to make a very acute

fever; but, even in this extreme case, the merely irritative type, though exaggerated, is essentially preserved.

As different sorts of animals are notoriously liable to different sorts of parasites, so, even among animals of one sort, as, for instance in the human kind, the liability of different bodies to receive particular parasite-infections does not seem to be quite equal for all. Especially, the vegetable parasites seem to have their affinities determined or modified by the general state of health of the recipient; and there are cases in which it looks as if there ran in particular family-lines (perhaps with some slight chemical idiosyncrasy) a special liability to particular intestinal worms. There, however, is no reason to believe that in regard of the more important animal parasites, as particularly of trichina and the tæniadæ, the susceptibility of individuals to attacks is other than universal and practically equal.

B.—The true or Metabolic Contagia (to which the rest of the present article will exclusively refer)—the contagia which, in their respective and specific ways, operate transformingly on the live bodily material which they affect, are, perhaps, the most important of all the incidental physical influences which concern mankind. Whether they may all, at some time hereafter, admit of being named, like the parasitic contagia, in terms of biological classification, is a question which needs not in the first instance be raised; for meanwhile the identity of each separate true contagium is settled in experimental and clinical observation by the uniformity of the operation of each on any given animal body which it affects. Each of the diseases propagates itself in its own form in as exact identity as if it were a species in zoology or botany; and in each such repetition of the disease there is a multiplication—always a large, and sometimes an inconceivably immense multiplication—of material which has the same infective property. Evidences innumerable to that effect are under daily clinical observation in this climate in instances of small-pox, measles, scarlatina, whooping-cough, enteric fever, mumps, typhus, syphilis, cow-pox, diphtheria, erysipelas, hospital-gangrene, purulent ophthalmia and gonorrhœa, venereal soft chancre and phagedæna, etc.: for, barring fallacies, no man ever sees any one of those diseases produced by the contagium of any other of them; and any man who has before him a case of any of them can see that, however minute may have been the quantity of contagium by which the disease was started, the patient's diseased body (part or whole) yields for the time an indefinitely large supply of the specific agent. It is more or less habitual to some of the diseases that the infectedness of the patient is first made known to the observer by such general pyrexia as tells of change already far advanced in the circulating mass of blood; and it

is only after this has shown itself that other symptoms, adding themselves to the fever, complete the more or less complex type which establishes the identity of the disease. But in many of the diseases it may be the case (either naturally or as result of experimental infection) that the first, and in some diseases the main or even the only, effects of the contagion are local changes, passing where we can from the first observe them; and the broad facts of metabolic infection, as regards waste of bodily material with concurrent increase of contagium, are, in many such cases, among our most familiar experiences. Most instructive, too, are the facts of contagion which are to be learnt in the study of tubercle: the contagium, introducible either by tubercular and certain septic inoculations through the skin, or internally by the infective action of the milk or diseased organs of tubercular animals if taken as food; and the contagium, when introduced, gradually spreading as it multiplies, and as lymph and blood carry it from the first infection-spot to other organs which now will repeat the process. And similarly in cancer (though the primary disease is at present of unknown origin and cannot be created by experiment) the repetition of the primary disease in secondary and tertiary propagations in the body of the sufferer is one of the most striking of all evidences of contagion: because of the great number of structural types which pass under the name of cancer, and the fidelity with which each of them is reproduced in the organ to which the contagion extends. A further fact of contagion, deserving notice in the present context, is the local spread of certain of these processes by continuity of tissue; as, for instance, in the continuous extension of phagedæna or hospital-gangrene from any centre of first inoculation, or of tubercular softening or cancer at the place where it begins: a mode of extension which indicates successive infective actions of matter on matter in spheres of ever-widening circumference; and the like of which, but in rudimentary degree, may be traced in the areola of any acute inflammation.

In the physiology of the metabolic contagia, no facts are more characteristic or more important than those which show the relativeness of particular contagia to particular receptivities of body. First, and in intimate connection, as would seem, with a chemical electiveness of action which will presently be imputed to contagia, there is the preference which some particular contagia (however introduced into the system) show for particular organs of the body; so that, by the exercise of this preference, there is given to each of the diseases its own set of clinical and anatomical characters. Compare, as instances in this point of view, the respective local affinities of small-pox, enteric fever, mumps, syphilis, hydrophobia, etc.—Secondly, it may be noted that, in regard to some of the contagia, different

persons, and particularly persons of different family-stocks, show original differences of susceptibility; original, namely, as distinguished from others, hereafter to be mentioned, which are acquired; so that, for instance, the severity with which scarlatina or diphtheria will strike in particular families contrasts with a comparative mildness of the same disease in other families or perhaps even with cases of apparently complete personal immunity under exposure to the particular danger: and recent researches have seemed to suggest as possible that, in the very wide differences of degree with which tubercular disease prevails in different families, an essential condition may be, that the families have widely different degrees of original predisposition towards some of the septic contagia.—Thirdly, there is the extremely suggestive fact with regard to many of our best-known febrilising contagia, that they run a course of definite duration, and that in this course, provided the patient do not die, all present, perhaps all future, susceptibility to the particular contagium is utterly exhausted from the patient; so that reintroduction of the same contagium will no more renew that patient's disease than yeast will excite a new alcoholic fermentation in any previously well-fermented bread or wine. The inference from this fact seems unavoidable, that each such contagium operates with a chemical distinctiveness of elective affinity on some special ingredient or ingredients of the body; and that exhausting this particular material in febrile process, which necessarily ends when the exhaustion is complete, is the bodily change which the contagium "specifically" performs.—Of not all metabolic contagia, however, can it be said that their operation runs so definite and self-completing a course. For, first, there are particular acute infections which, as a rule, kill; either (as appears to be the case in splenic fever when affecting man) because of the extreme magnitude of the transforming process which the contagium sets up, or else (as appears to be the case in hydrophobia) because the elective incidence of the contagium is on an organ indispensable to life; so that in such cases there is in fact hardly such an event as passing alive through the whole process of the disease. And, secondly, there are the contagious dyscrasies, which are clearly characterised by their tendency to indefinite duration: syphilis, which oftener than not relapses in successive outbreaks, and often as years pass invades the body more and more deeply, and may after all never during life be ended; and tubercle and cancer, which, with almost invariable persistence, will in general steadily advance month by month to infect more and more of the body till the process eventuates in death.

The transmission of various contagious diseases in communities is of course greatly influenced, both in detail and in

aggregate, by such differences of individual receptivity as were mentioned in the last section. Notably, as regards communities through which particular acute infections have had full run, fresh sparks of the contagium may find little or no fuel on which to act; and much new diffusion of the disease may not again be possible, till immigration, or births, or lapse of time operating in other ways, shall have reconstituted a susceptible population. And, given the susceptible population, circumstances of time and place are infinitely various (especially as regards quantity and quickness of personal or quasi-personal intercourse) in determining how far this population shall have particular contagia thrown in its way.

Also there are conditions, not primarily of a personal kind, which operate on a very large scale in determining the spread of some of the metabolic infections; giving to them respectively at certain times, in ways not hitherto understood, a special increment of spreading power, and in some instances also special malignity: and thus enabling them respectively from time to time to come into comparative prominence in national life, and perhaps at once or successively in many different countries, in the form of so-called Epidemics. Thus, it is matter of familiar knowledge that the fevers which are most habitual to this country, scarlatina, measles, small-pox, enteric fever, are of nothing like uniform prevalence,—that scarlatina, for instance, will be three times as fatal in one year as in another, and that small-pox is liable to even greater exacerbations: and it is known that temporary differences of this kind are not exclusively local,—that for instance (to quote a late official report) "the epidemic of small-pox which began in England towards the close of 1870, and terminated in the second quarter of 1873, was part of a general epidemic outbreak of that disease, of world-wide diffusion, marked wherever it occurred by an intensity and malignity unequalled by any previous epidemic of the disease within living memory." The wider the survey which we take of epidemiology, the more certain it becomes to us that, outside the conditions which are independently personal or local, there are cosmical conditions which have to be considered. Doubtless, there are great epidemiological facts—such, for instance, as the first spreading of small-pox to America, or in our own times the increasing frequency of Asiatic cholera in Europe, which may be ascribed to novel conditions of international intercourse: but there are others, equally great, to which apparently no such explanation can be applied. For what reason it is that cholera every few years has its definite fit of extension in India,—or why diphtheria, which scarcely had a place in history till it overran Europe in the sixteenth century, and which since then had been rarely spoken of, began

again some twenty-odd years ago to be comparatively important in England,—or why the plague of the Levant has for the last two centuries been so unfamiliar to us,—or why the yellow fever of the Mississippi has in particular years raged furiously in parts of Europe,—or why our black-death of the fourteenth century, though apparently still surviving in India, has never but that once been in Europe,—or whither has gone our sweating-sickness of three centuries ago—or whence have come the modern epidemics of cerebro-spinal meningitis:—these, and many like questions, which cannot at present be answered, seem to be evidence enough that, in the making of epidemics, contagion and personal susceptibility may be factors in a partly conditional sense. Influences which are called "atmospheric"—the various direct and indirect influences which attach to the normal succession and occasional abnormality of seasons, in respect of the insolation of our planet, and of the temperature and humidity of air and earth, are in general far too vaguely regarded as elements of interest in the present question, but are possible factors which no epidemiologist should omit from scientific consideration.

In the passage of the metabolic contagia from person to person, various agencies may be instrumental,—bedding or clothing or towels which have been used by the sick, dirty hands, dirty instruments or other utensils, the washer-woman's basket, foul water-supply, stinking house-drains, contaminated milk or other food, the common atmosphere, etc.; but differences of that sort are only differences as to the means by which such communication is established with a diseased body as brings its products into relation with healthy persons; and the disengagement of infectious products from the bodies of the sick is pathologically the one influential fact. As regards the products which ought to be deemed infectious, the specially-diseased surfaces and organs of the patient, and the discharges and exhalations which they respectively yield, must always be regarded with chief suspicion; but suspicion, however much it may insist on them, must never disregard other sources of danger. Of some of the metabolic contagia we practically know, and of many of the others we may by analogy feel sure, that, when a given body is possessed by one of them, no product of that body can be warranted as safe not to convey the infection. Presumption against every part and product of the diseased body is by everyone readily admitted where there are vehement general symptoms of disease; but it is important to know that not only in such febrile states, but even in states of chronic dyscrasy, and even at times when the dyscrasy may be giving no outward sign, the infected body may be variously infective. Thus, in regard to constitutional

syphilis, it is certain that the mere utero-catarrhal discharge of the syphilitic woman, or the sperm of the syphilitic man, or the vaccine lymph of the syphilitic infant, may possibly contain the syphilitic contagium in full vigour, even at moments when the patient, who thus shows himself infective, has not on his own person any outward activity of syphilis. Similarly, in regard to tubercular disease, experiment has proved beyond question that the milk of animals suffering from tubercle will, if taken as food by other animals, infect them through the intestinal mucous membrane; and there are independent reasons for believing that the tubercular contagium (like the syphilitic) will at times during the dyscrasy be contained in the seminal fluid, and that men, tubercular, perhaps only in some degree which is not immediately important to themselves, may by that secretion convey fatal infection to women with whom they have conjugal relations. Regarding many of the metabolic contagia, conclusive evidence exists that, when they are in operation in pregnant women, the fœtus will in general be infected by them; and this though the diseases (*e.g.*, small-pox, cholera, syphilis) be of the most different pathological types; but, with regard to pregnant animals affected with splenic fever, it is noticeable that Brauell, in his extensive researches, found the blood of the fœtus not to be infective.

In general, each contagium has its own favourite way or ways of entering the body; and these preferences are not only of speculative interest, as attaching to varieties of nature and natural habits among the contagia, but are of obvious practical importance as measures of the widely different degrees in which the different contagia are qualified to spread in communities. Thus, inoculation at broken surfaces of skin or mucous membrane has long been known as the ordinary mode by which the infections of syphilis, hydrophobia, splenic fever, cow-pox, and farcy or glanders, get admission to the body; and our best knowledge of some other infectious diseases (notably of tubercle) has been derived from inoculations intentionally made with their contagia for purposes of study. While probably all infections which tend to be of general action on the body can be brought into action in that way, and while some infections are not known to pass by any other mode of transmission, there are many infections which spread freely from subject to subject by atmospheric and dietetic communication; and the meaning of these preferences is hitherto not fully known. It seems that some contagia are so acted upon by air and water that they seldom or never reach the body in an effective state by those common means of communication,—some hardly, if at all, by water, and some not by air except with very close intercourse; and further, that, of contagia which reach the body in an effective

state, some require, while others do not require, that an abnormal breach of surface shall give them special opportunity for taking hold. In some of the cases where a disease can be propagated in both ways—*i.e.*, certainly in small-pox, and apparently also in bovine pleuropneumonia, the artificially inoculated disease tends to be much milder than the disease otherwise contracted; but pathologically it is difficult to conceive any essential difference between those different modes of contagion. It may be presumed that, in the modes which are not by true inoculation, acts which are comparable to inoculation take place on internal surfaces; that, for instance, when particles of scarlatina-contagium are caught in the tonsils, and inhaled into the bronchi, and swallowed into the stomach, they begin by penetrating the texture of the mucous membrane, and by thus effecting as real an inoculation, with regard to the blood, as that which art or accident provides in other cases through the punctured skin. That previous abnormal breach of surface by artificial puncture or otherwise is not necessary to allow the infection of mucous surfaces is illustrated in ophthalmia and gonorrhœa; where apparently no other condition has to be fulfilled than that a particle of the blennorrhagic contagium shall be deposited on the natural surface of the mucous membrane. It deserves notice that, while a considerable number of the worst diseases of the domestic animals admit of being communicated to man by artificial inoculation, atmospheric communication seems to be very inapt, if not absolutely unable, to infect man with any one of them; and in this connection it may be of interest to remember that syphilis, one of the most familiar of human infections, but hitherto not traced to any brute ancestry, differs from our other current infections in requiring inoculation to transmit it.

When any metabolic contagium enters the animal body, it requires an interval of time, and in most cases a considerable interval, before its morbific effects can become manifest even to skilled observation. The period of latency or so-called incubation varies greatly in different cases. In hydrophobia, it is very rarely less than of one month, is certainly often of several months, and is said to be sometimes of years. In syphilis, the inoculated spot remains generally for at least a fortnight, and may remain even as much as five weeks, without any ostensible change; and the roseola of the general infection will not be seen till some weeks later, when generally at least three months will have elapsed since the first inoculation. In the acute eruptive fevers, when their contagium is transmitted by air, the first changes which ensue on infection are not external, and we

cannot be sure what early internal changes may take place; but in small-pox, the fever (which is the first overt sign) does not attract notice till about the twelfth day after infection, nor the eruption till two days later; and in measles the incubation time, though perhaps less uniform, seems to be little (if any) shorter than that of small-pox. The septic contagia and the contagium of splenic fever seem to be of particularly quick operation; but even the most virulent septic contagium, when without admixtures which tend to complicate its action, will not begin sensibly to derange the infected animal till at least several hours after it has been inoculated. As regards the contagia last referred to, it is conceivable that the self-multiplication of the contagium in the form in which it proves fatal to life is a process which goes on continuously and uniformly from the moment of inoculation to the moment of death, and that the moment when signs of general derangement become manifest is the moment when this uniformly advancing process has accumulated in the system a certain quantity of result; but it does not seem easy to apply this explanation to the diseases of long incubatory period; and we can hardly conjecture what may be the latent processes—for instance, of small-pox, during the first ten or more days after contagium has been received.—*British Medical Journal, Dec.* 13, 1879, *p.* 923.

2.—ON THE TREATMENT OF TYPHOID FEVER.

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I have never known a case of typhoid fever cut short by any remedial agent—that is cured. The poison which produces any one of the acute specific diseases (to which order typhoid fever as much as small-pox belongs) having entered the system, all the stages of the disease must, so far as we know, be passed through before the recipient of the poison can be well. If the patient can be kept alive for a definite time the specific disease ends, and then, if no local lesion remains to constitute a substantive disease, the patient is well. The treatment of typhoid fever is essentially rational. To treat a case with the best possible prospect of success the physician ought to be acquainted with the epidemic constitution of the period, the etiology of the disease, its mode of attack, its natural course, the order of appearance, and the natural duration of each of its symptoms, the way in which each symptom influences the termination, the several pathological lesions which produce or may produce each special symptom, and the complications to be watched for at each stage of the disease.

Certain facts have to be kept in mind when treating a case of typhoid fever, and when estimating the effect of remedies: First, that the disease, in the majority of cases at least, is produced by the action of a small portion of the excreta from the bowel of a person suffering from typhoid fever; that air from a drain, or air blowing over dried feculent matter, may convey the poison to the patient, or his own fingers may carry it to his mouth, or that the vehicle for the poison may be a fluid—for example, water or milk; and that the poisonous properties of the excreta may be destroyed by boiling the fluid in which they are contained, though not by filtering the fluid. Secondly, that the natural duration of a well-developed case of typhoid fever is from twenty-eight to thirty days. Hence subsidence of the fever before that date should be regarded with suspicion, and the patient not treated as if the specific disease had ended, while the continuance of febrile disturbance after that date should lead to repeated and most careful examination of the patient, in order to ascertain if any local lesion is keeping up the febrile excitement. But not only is the duration of the specific disease limited, but the prominent symptoms have their regular order of sequence, and several their own natural limits of duration—that is, a time to begin and a time to end; and the natural termination of a symptom has often been attributed to the remedial agent last employed.

In the earliest stage of typhoid fever the patient is prone to commit certain mistakes in treating himself, either of which may add greatly to the severity of the coming illness. (1) He may think that he has a common cold in his limbs, as it is called, and try to throw it off by strong exercise. A certain sense of weakness accompanies this early stage of the fever, but it is rarely so great as to prevent the patient, if stimulated by strong will, walking long and briskly. (2) He may consider that he is suffering from biliary derangement, and attribute to this the headache, disturbed nights, sense of malaise, want of appetite, and disordered bowels, and take a dose of drastic aperient. (3) He may think the weakness he feels is to be removed by food and wine. A dose of medicine, he says, cannot hurt; bed, he thinks, weakens; and food and wine, he knows, restore strength; therefore he prescribes a dose for himself which irreparably injures his bowel; he takes exercise which increases the waste material in his system, and he loads his stomach with food it cannot digest and stimulants which heighten the fever and disturb the action of the eliminating organs, and then pays the penalty, perhaps with his life, for the errors his ignorance has led him to commit.

The thermometer saves, or ought to save, the physician from the patient's mistakes. The temperature having rendered it

even *possible* that the ill-defined and may be trifling symptoms are due to the poison of typhoid fever, the patient should be absolutely confined to bed. Exercise or fatigue immediately before or after the absorption of the poison, and especially when the action of the poison is manifesting itself, increases the gravity of the disease, partly by exhausting the nervous power, but chiefly, I think, by causing destruction of tissue, and so the presence of waste material in the system on which the poison can act; and at the same time, by disturbing the action of the eliminating organs, it leads to the retention in the system of the products of the destroyed tissue.

Some of the worst cases of typhoid fever I have ever seen have appeared to me to owe their gravity to the patient having travelled, after the commencement of the sense of illness, in order to reach home. I very rarely advise a patient's removal to his home, if that be distant, so satisfied am I that the fatigue of travel, whether by rail or carriage, tends to make what would otherwise have proved a mild case severe, and to cause a bad case, which might after perhaps a struggle have ended favourably, to terminate in death. Not only is other tissue-destruction than that due to the fever process in a great degree prevented by rest in bed, but the nervous system is there less liable to disturbance, any tendency to moisture of skin is favoured, the elimination of the products of waste tissue is unchecked, and chances of error in diet diminished. The air of the room should be as pure as possible; the room in which the patient is placed be large enough to permit it being freely ventilated without draughts; if possible, the patient should occupy a different room at night from that used in the day.

From the first the patient should be restricted to liquid diet, with farinaceous food, and bread in some form if the appetite requires it. It is better to vary the broths, and to add to them some strong essence of vegetables. Sometimes a little strained fruit juice is taken with advantage, but skins and seeds of fruits and particles of the pulp are frequent sources of irritation of the bowel. Grapes are always dangerous from the difficulty of preventing the seeds slipping down the throat. The value of milk as an article of diet in fever is generally admitted, but it requires to be given with caution. The indiscriminate employment of milk in almost unlimited quantities as diet in fever has led to serious troubles. Milk contains a large amount of solid animal food. The casein of the milk has to pass into a solid form before digestion can take place. Curds form in the stomach, and, the digestive powers being weakened in fever, these curds may remain unchanged in the stomach, and produce considerable disturbance of system.

I have seen the patient restless, sleepless or drowsy, his temperature raised several degrees above what it had previously been, vomit, eject a quantity of curd, and at once the restlessness cease, the temperature fall, the skin become moist, and the patient drop into a quiet sleep. All the threatening symptoms vanish with the ejection of the offending material. Or the undigested curds may accumulate in the bowel, inducing flatulent distension and pain in the abdomen, restlessness, and increased febrile disturbance. Under these circumstances, I have seen an enema of thin gruel bring away a large vesselful of offensive, sour, undigested curds. Or, again, the undigested curds may themselves (and this has not been an uncommon consequence of milk diet in my experience) irritate the bowels, and produce, keep up, or greatly increase diarrhœa. A distinguished chemist once remarked to me, "Do not forget that a pint of milk contains as much solid animal matter as a full-sized mutton chop"; and solid the casein of the milk must become before it can be digested; and yet I have known a patient drink two quarts and even more of milk in twenty-four hours—*i.e.*, solid animal food equal to four mutton-chops. Can anything approaching to such an amount of solid animal food be digested, and, if it could, is such an amount of animal food good for a patient suffering from typhoid fever? He is weak because of the presence of the fever, and not from lack of food.

Patients suffering from typhoid fever should be allowed an unlimited supply of pure water. When pure water is freely absorbed, it passes away by the kidneys, skin, lungs, &c., and is of much service as a depurating agent. If it be *possible*, even, that the poison of the fever was conveyed into the patient by the drinking-water or the milk of the district in which he is ill, then these fluids should be boiled till a different supply is obtained. All sources of foul air from drains or cesspools should be sought for, and the air the patient breathes be freed from all possibility of impurity; disinfectants should be placed in the close-stool, and the dejecta buried if possible. If the bowels are confined in the early stage of the disease a simple enema should be given. Hard stool retained in the bowel will produce irritation, and it may be catarrhal inflammation of the intestinal mucous membrane, and so induce troublesome diarrhœa. Small doses of mineral acid, well diluted, are grateful to the patient, and may perhaps be useful.

The fever is thus met by rest, quiet, fresh air, mixed liquid food and bland diluents, and the exclusion of fresh doses of poison; the intestinal lesion by the careful exclusion from the diet of all hard and irritating substances, and the removal from the bowel of any local irritant.

Frontal headache and sleeplessness are two symptoms of the early stages of typhoid fever which may cause the patient much distress. The headache is in some cases alleviated by cold and in others by warm applications; but as the headache ceases spontaneously in about ten days from the outset of the fever, no active treatment is required for its cure. Local applications, the exclusion of light from the room, and absolute quiet, have been all the means I have seen necessary for its relief. Sleeplessness is a more important symptom, for although it also usually disappears or diminishes spontaneously during the second week of disease, still this is not invariably the case; and the nervous system may be greatly worn by the want of sleep, lasting as it does occasionally, unless treated, night and day. The drugs I employ when, from the continued sleeplessness, it seems absolutely necessary to relieve it, are henbane, bromide of potassium, and chloral. From a combination of the latter I have seen very good results, and, so far as my experience has gone, no really ill effects when its use has been limited to the earlier stages of the disease—*i.e.*, to the period anterior to signs of nervous prostration. If the patient's temperature be high, a tepid bath, or sponging the surface will often at once induce sleep, and no drug be required. In the earlier and also in the later stages of typhoid fever the sleeplessness has been treated by opiates. Experience has convinced me that although, in some cases, opium in sufficient dose to secure sleep has afforded relief, it is on the whole a most dangerous remedy. In the early stage of fever it disturbs digestion and checks secretion. In the later stages of the disease I have seen several cases fatal from its influence on brain, heart, and secreting organs. To my mind, the hoped for and occasionally attained good is altogether outweighed by the disproportionately possible evil and occasionally fatal effect resulting from the administration of direct sedatives, opium and chloral, in the later stages of fever.

So long as the stools do not exceed three to five of moderate quantity in twenty-four hours, the looseness of bowels is rather advantageous than injurious; but when, from their number or from their quantity, there is risk of the strength of the patient being reduced to a dangerous degree, then it becomes necessary to restrain the diarrhœa. The treatment varies with the pathological condition which induces the diarrhœa. It is often sufficient to examine the stools to detect the cause and remove it—*e.g.*, curds of milk. Inquiry may prove the great excess of the fluid taken into the stomach over that passed by kidneys and skin. When the stools are strongly alkaline diluted sulphuric acid sometimes affords marked relief. When the stools are merely frequent four ounces of starch water thrown

into the rectum night and morning will often check the frequent action. Should this not prove efficacious, from three to ten drops of laudanum in one ounce and a half of starch water may be thrown into the bowel night and morning *after* the passage of the stool. When very offensive, correctives of fetor should be given; a teaspoonful of charcoal may be given two or three times a day. Under its influence I have seen the stools lose their fetor and ammoniacal odour, become less liquid, and in many cases the irritation from the catarrhal inflammation of the mucous membrane diminished. The greatest possible care should be taken that the charcoal is an impalpable powder; animal charcoal has, in this respect, some advantages over vegetable. Other correctives of fetor, or antiseptics, will have as good effect as charcoal, but this has given me such satisfactory results that I have not resorted to other remedies of its class.

Carbonate of bismuth, in twenty-grain doses every four or six hours, is one of the best remedies I know for the catarrhal inflammation of the bowel itself. If the fluid poured out be very excessive, then a vegetable astringent, as catechu and kino, may be given with the bismuth, and, should these means fail, three to five drops of laudanum must be added to each dose of the mixture. The use of opium, even in this dose, by the mouth, should be avoided if possible, as, by interfering with the action of excreting organs, and by its other effects on the nervous system, it may do more harm than the symptom for which it is prescribed. The administration of opium should if possible be limited to the bowel.

When blood in ever so small a quantity is observed in the stools, the patient is to be kept in the recumbent position. He should not be allowed to make any effort, and on no consideration to sit on close-pan or raise himself or be raised by others to pass urine, &c. If the patient is unable to pass urine when in the recumbent position, the urine should be drawn off by catheter. All movement of the bowels should be restrained as far as possible and for as long as possible. An ounce of starch-water with ten or fifteen drops of laudanum may be given at once as enema, and then a dose of acetate of lead every two or three hours by the mouth, with three to five drops of laudanum, or fifteen grains of gallic acid in a wine-glass of iced water every two or three hours with the laudanum. I think I have seen benefit from small doses of turpentine. It is a point of the greatest moment to keep the bowels empty, and therefore nourishment should be given in the most concentrated and absorbable form—*e.g.*, essence of meat in tablespoonful doses frequently repeated. All nourishment which leaves solid residue—*e.g.*, milk—should be avoided.

Lumps of ice should be sucked, and all essence of meat, &c., be iced. When the loss of blood is sudden and copious, or is frequently repeated, the subcutaneous injection of ergotine may be employed, and an ice-bag applied over the region of the ileum. The faintness due to considerable sudden bleeding from the bowel should not be removed by stimulants unless it immediately threatens life.

From the commencement of typhoid fever the temperature of the patient is elevated. This elevation of temperature is the direct result of excess in destruction of all the tissues of the body. The patient loses weight, and continues to lose weight so long as he is the subject of the fever, so that Zimmerman fixed the duration of typhoid fever by ascertaining how long after the onset of the disease the patient ceased to lose weight. In local inflammation the temperature of the inflamed part is greatly in excess of the temperature of the body generally. In uncomplicated typhoid fever the elevation of temperature is practically uniform in all parts of the body. Local inflammation, blood-infection, and nerve-disturbance, are common causes of great increase of the temperature above that *proper* to any given case of typhoid fever—*i.e.*, due to the action of the fever poison on the system generally. Thus inflammation of the mesenteric glands is frequently excited by fetid or acrid matters conveyed to them by the lymphatics from the raw and inflamed intestine, and blood-infection may owe its origin to the same cause. Congestive nephritis and pneumonia are common lesions in typhoid fever, and are common causes of increase of temperature. The power of the nervous system to raise the temperature, or to produce sudden changes which raise the temperature, in typhoid fever, is well illustrated by the sudden rise which frequently follows mental emotion. I have known the temperature start from this cause from 102° to 106°. In the progress of typhoid fever local inflammations cause more or less permanent elevation of temperature; blood-infection rapid, and sometimes temporary, always varying, elevations of temperature; nerve-disturbances rapid and often transient elevations of temperature.

The treatment of typhoid fever by cold baths, when the temperature reaches 104°, or even less, is very generally adopted in Germany; but neither my own limited experience nor the evidence adduced by others in its favour has carried conviction to my mind of its advantage. At the same time I entertain no doubt that the direct cooling of the body is in some cases essential to the preservation of the life of the patient—gives him, so to say, his only chance of recovery; while in others it alleviates the severity of a symptom which increases the danger of the patient.

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A certain degree of elevation of the body is incompatible with the favourable progress of the case. Thus a temperature of 106°, rising to 107°, and still advancing, will, unless lowered by treatment, speedily end the life of the patient; for these fortunately very rare cases the direct and rapid depression of the patient's temperature by the cold bath is essential—is the only source of hope. Again, a temperature of 105°, if continuous for forty-eight hours, or broken only by a slight fall of short duration, may itself, by the degenerative changes it favours or produces in the heart and other organs, cause the fatal termination; for these cases a tepid bath gradually cooled down is sufficient to bring down the temperature; the bath has to be repeated as the temperature rises. Persons at an age when degenerative changes of tissue naturally occur suffer more from continued high temperature than do younger persons. If the disease be advanced, the skin dry, the patient prostrate, and the internal organs congested, especially the kidneys, a tepid or warm pack is often followed by marked improvement in temperature and in the other symptoms. When the temperature is for some time 103½° to 104½, and even higher, cold effectually applied to the head (*e.g.*, by the india-rubber tubing cap) will often suffice for its reduction—reduction, that is to say, not only of the temperature of the head, but of the body generally; when the temperature falls the cap should be removed, but be reapplied as often as it rises again. Tepid sponging will reduce the temperature a little, and soothe and quiet the patient if he be restless and sleepless.

Thus limiting the employment of direct means of lowering temperature, I have seen none of the ill effects which are said occasionally to follow its vigorous use—viz., collapse and failure of the heart's power; while I have seen what has appeared to me to be indisputable good follow its employment.

Other means are used to reduce temperature. When the high temperature is conjoined with rapid feeble heart's beat, when the patient is advanced in years, or when the extremities feel cold, though the temperature of the body generally is high, a fall of temperature frequently follows the administration of alcohol. Quinine, in occasional large doses or in smaller doses repeated at intervals, has been followed by reduction of the temperature in typhoid fever. Salicylate of soda is also said to have a like effect. When judging of the effects of these and other drugs for reducing temperature, one must not lose sight of the fact that sudden fall of temperature when no drug is given is of not unfrequent occurrence, and also that the temperature varies daily. This natural fall of temperature I have seen attributed to the drug. I must say that I have been disappointed in the effects of the two drugs I have named as

reducers of temperature, while I have seen both occasionally do much harm by disturbing the stomach and interfering with digestion. In a disease which runs a limited course, like typhoid fever, the greatest possible care should be taken to preserve the powers of the stomach, as the life of the patient may depend on his power to digest nourishment towards the end of the disease.

Typhoid fever cannot be cured by perspiration, but the patient suffers much less discomfort and is less restless when the skin is gently moist. The most certain means for producing gentle perspiration I know is the application of a large warm and moist flannel covered with oil silk over abdomen and chest, and the administration of warm bland fluids. In the later stages of the disease, profuse and most exhausting perspiration sometimes occurs. The perspiration is kept up and greatly increased by the heat of the patient's body and the moisture of the perspiration itself. To prevent chill, the nurse carefully covers the patient; thus, with a relaxed skin disposed to sweat, he is kept, so to say, in a prolonged vapour bath. I have seen lives lost from the asthenia thus produced. To restrain this profuse perspiration, the patient's skin should be dried with a warm napkin every few minutes if necessary, and dry clothes placed between the wet linen and the skin. Alcohol should be given at the same time to stimulate the nervous system and increase the force of the heart's action. Sponging with tepid vinegar and water is sometimes of much service.

Delirium is, as a rule, one of the symptoms which are influenced for good by alcohol, but it is one in which the effects of the alcohol should be most closely watched. When alcohol is contra-indicated or proves useless or injurious, the head should be shaved and cold applied either by an ordinary ice-bag or by the cold water tubing cap, or the head may be kept cold while the force of the heart's action is maintained by alcohol.

There is one nerve symptom to which I attach great and grave significance, and which is a guide to treatment—it is tremor.

Tremor is sometimes so great that the patient is unable to protrude his tongue; his jaw trembles, and his voice quivers; and if he attempts to hold any vessel, he spills its contents, not from weakness or drowsiness, but from shaking of the hands. Tremor out of all proportion to other signs of nervous prostration is evidence of *deep* destruction of the intestine. A small *deep* slough will be accompanied by great tremor; a large extent of superficial ulceration may be unattended by symptoms. Now it is deep ulcers following separation of deep sloughs which are especially liable to give rise to severe hemorrhage and perforation. In these cases of tremor alcohol should

always be given to increase nerve energy, and so to limit the sloughing and ulceration to what is the direct consequence of the specific process.

I may sum up my experience in regard to the use of alcohol in the treatment of typhoid fever thus. Its influence is exerted primarily on the nervous system, and through it on the several organs and processes; for example, the heart and the general nutritive processes—changes on which the rise and fall of temperature depend. In judiciously selected cases it lowers temperature, increases the force and diminishes the frequency of the heart's beats; it calms and soothes the patient, diminishes the tremor, it quiets delirium, and induces sleep. It should never be given in the early stage of the disease, or with the hope of anticipating and so preventing the occurrence of prostration and debility, but should be prescribed only when the severity of special symptoms, or the general state of prostration, indicates its use. Hence a large number of cases of typhoid fever end favourably without alcohol being prescribed from the beginning to the termination. It should not be prescribed when a sudden gush of blood has induced faintness, unless the faintness is so great as to threaten life immediately. Nor should it be given when, after the first few doses, the temperature rises, the heart's action becomes more frequent or more feeble, delirium increases, sleeplessness supervenes, or drowsiness deepens, so as to threaten to pass into coma. When the urine contains a considerable quantity of albumen, alcohol should not be prescribed, unless absolutely necessary for the relief of some symptom immediately threatening life, and then it should be given with the greatest caution, and its effects on temperature, the circulation, and on the urinary and other secretions, both as to quantity and quality, be carefully and frequently noted. The quantity of alcohol prescribed should be as much only as may be necessary to effect the object for which it is prescribed. In the fourth week, to tide the patient over the concluding days of the disease, it may, as a rule, be given more freely than in the second or the beginning of the third week of the disease, but it is in exceptional cases only that more than twelve ounces of brandy in the twenty-four hours can be taken without inducing some of the worst symptoms of prostration. Nearly all the good effects of alcohol, when its use is indicated, are obtained by four, six, or eight ounces of brandy in twenty-four hours. Taken in excess—even when in smaller quantities it would do the patient good,—it dries the tongue, muddles the mind, or induces delirium or drowsiness approaching to coma, and diminishes the action of the secreting organs, on the healthy action of which the elimination of the materials destroyed by the action of the fever

poison depends. For the last thirty years I have made it the rule of my practice in the treatment of typhoid fever to abstain from giving alcohol if, in the case before me, I *doubted* the wisdom of giving it; when in doubt I do not give alcohol in typhoid fever, and when there is a question in my mind of a larger or smaller dose I, as a rule, prescribe the smaller. The reverse of the rule I laid down for myself in the treatment of typhus fever.—*Lancet, Nov.* 15, 1880, *p.* 715.

3.—ON SOME POINTS IN THE PATHOLOGY AND TREATMENT OF TYPHOID FEVER.

By Dr. WILLIAM CAYLEY, Physician to the Middlesex Hospital and to the London Fever Hospital.

What are the nature and properties of the poison which is supposed to give rise to the disease?

Now, the typhoid poison has up to the present time eluded all attempts to isolate it or to demonstrate its nature either by microscopical examination or chemical analysis; we are only conscious of its existence by the effects which it produces on the human organism. Nevertheless, some of its properties are known with tolerable certainty: (1) when introduced into the system it multiplies; (2) it is contained in the alvine discharges of persons suffering from the disease; (3) it retains its activity for an indefinite time after it has passed out of the body, when placed under favourable conditions, these conditions being the presence of decaying animal matter and moisture. Hence its usual habitats out of the body are drains, sewers, cesspools, dung-heaps, wet manured soils. And there are some grounds for supposing that in these situations also it may possess the power of multiplying. Lastly, in all probability it is particulate, and not either liquid or gaseous.

The actual nature of the poison—whether it be, according to the hypothesis most generally accepted, some kind of fungus, or microzyme, or protoplasm, in a word, a *contagium vivum*, or whether, as maintained by others, it is some derivative of albumen, capable of exercising a catalytic action on other albumen, —I do not propose to discuss, as it is a question at present rather of theoretical than practical interest, and it is one, moreover, for whose final determination the data are hardly yet sufficient.

A subsidiary question to this, but one of considerable practical importance, is whether the poison can be generated *de novo* from decaying organic matter, whether it be pythogenic, as was so ably maintained by Dr. Murchison, or whether it can only arise by continuous propagation, as was maintained no less ably by Dr. William Budd.

The arguments on both sides may be very briefly stated.

In favour of its origin *de novo* it is asserted that typhoid fever has often broken out in isolated situations—as solitary farm-houses, far removed from, and holding no communication with, places where the disease exists; and many such instances are given by Dr. Murchison. On the other hand, it has been proved incontestably by many instances, both in this country and on the Continent, that all the conditions supposed to be required for its generation may be present for an indefinite time—as percolation of sewage into wells supplying drinking-water,—and yet the disease does not show itself till the poison is introduced by the arrival of an infected person, when an outbreak at once takes place.

Now, I would submit that this latter argument far outweighs the former; for otherwise, if it be proved that all the conditions necessary for the origination of the poison are present, as shown by its subsequent development when the germs are introduced, and yet it does not develope, we should have to admit that the same causes are not always followed by the same effects.

The instances in which persons in the latent stage of typhoid fever, or actually ill with it, have carried the disease to distant places, and caused it to spread, are so numerous that I believe that this mode of propagation is now universally admitted; the communication of the disease taking place not by direct contagion from the sick to those brought into immediate contact with them, but by the ordinary mode of sewage contamination.

The apparently spontaneous origin of the disease in isolated places may, doubtless, be in part explained by the very long time the poison may lie dormant, still retaining its essential properties, and capable under favourable conditions of developing its activity.

The poison, too, may be introduced in quite unsuspected modes. It is well known that the typhoid poison by no means always produces what is usually recognised as typhoid fever. In very many cases its only effect is to cause some malaise, together with slight intestinal catarrh, which is not necessarily attended by diarrhœa. These cases are only recognised as typhoid when they occur during an epidemic, and among persons who have been exposed to infection; but if we may judge by the analogy of scarlatina, diphtheria, and other contagious fevers, where similar slight unrecognisable forms are not uncommon, these apparently trivial cases should be as potent in communicating the disease as the severe well-marked forms. Hence it is quite possible that the typhoid poison may be introduced into a locality by a person whom no one would suspect of being infected with the disease.

It has been ingeniously suggested by my colleague, Dr. Robert King, that the typhoid poison is derived solely from decomposing albumen, which is not present in healthy stools, and that it can therefore only arise from morbid stools which contain albumen, as is the case in some forms of catarrhal diarrhœa, and he has published a case where the poison seemed to be generated in this manner. This theory might certainly account for long-continued sewage contamination of water without the production of typhoid fever, as the special material from which the typhoid poison is generated might be absent. But it is hardly likely that any such derivative of albumen would remain undecomposed in a drain or cesspool for so long a period as we have reason to believe is the case with the typhoid poison.

We have next to consider how long the typhoid poison can thus retain its activity out of the body in a suitable locality. One of the clearest pieces of evidence on this point is the well-known instance related by Dr. von Gietl. To a village free from typhoid an inhabitant returned suffering from the disease which he had acquired at a distant place. His evacuations were buried in a dunghill. Some weeks later five persons who were employed in removing dung from this heap were attacked by typhoid fever: their alvine discharges were again buried deeply in the same heap, and nine months later one of two men who were employed in the complete removal of the dung was attacked and died. Here we have distinct evidence that the poison retained its powers for nine months.

Dr. Murchison, in his work on Fever, gives an instance in which six cases were spread over a period of eight years. I have recently seen an instance in which an interval of two years occurred without apparently any fresh importation of the poison. Supposing the germ theory to be correct, there is, of course, no reason why the poison should not preserve its vitality, by continuous propagation, for an indefinite time.

On the whole, though the point cannot be regarded as finally determined, I think the weight of evidence is against the *de novo* origin of the disease.

We have now to consider a question about which much difference of opinion still prevails, viz., the contagiousness of typhoid fever. That the disease is communicable from the sick to the healthy is, I believe, universally acknowledged, the point in dispute being the mode in which the transmission takes place, whether directly by emanations from the patient, or from his fresh evacuations, or indirectly from eating or drinking, or inhaling the emanations from the stools, modified by their having undergone some kind of decomposition or

fermentation outside the organism. Although the point in dispute is a narrow one, it is of considerable importance, and erroneous views may lead, on the one hand, to the adoption of unnecessary restrictions, which may seriously incommode the patient and his attendants, while at the same time it is only too likely to cause a neglect of the really essential precautions.

I have myself witnessed an outbreak of typhoid where the belief in its contagiousness—shared, I may say, by the medical practitioners—was so strong as to excite quite a panic, so that difficulty was found in procuring attendants for the sick. In consequence, I had on one occasion myself to carry a patient from one room to another, and a man who volunteered to help me was regarded as having done something rather heroic. I need hardly say that, at the same time, the utmost recklessness is commonly shown with regard to the real causes of its dissemination.

On the other hand, a disbelief in the direct contagiousness of the disease might, if not well founded, lead to unnecessary exposure of the patient's friends.

The arguments against the direct contagiousness of the disease are in the main these:—1. In hospitals we rarely find that typhoid fever spreads either to the attendants on the sick or to the other patients. When it does thus spread, persons in other parts of the building, who have never been brought in contact with the typhoid cases, are attacked about as frequently as the immediate attendants themselves.

Dr. Murchison has published the evidence on this point afforded by the London Fever Hospital for a period of twenty-three years, up to 1870. During this period 5,988 cases of typhoid were admitted, and seventeen of the resident staff took the disease, but of these seventeen only five were in communication with the typhoid cases, and twelve occurred at a time when there were serious defects in the drains; twelve patients also admitted for other diseases became infected. But since 1861, when the patients were so classified that the typhoid cases and the patients suffering from other acute diseases, not fever, are placed in the same wards, not a single instance has occurred in which the infection has spread to the non-typhoid cases, though the same night-stools and water-closets have been used by both classes of patients, and the use of disinfectants has been exceptional. This shows pretty conclusively that the emanations from the recent stools are not capable of communicating the disease.—*Medical Times and Gazette, March* 13, 1880, *p.* 283.

4.—ON THE USE OF COLD BATH IN ENTERIC FEVER

By Dr. ALEXANDER COLLIE, Medical Officer to the Homerton Fever Hospital.

A study of the papers of Professor Liebermeister and others, on the use of the cold bath in the treatment of enteric fever, led me to try it on the cases which came under my care in the Homerton Fever Hospital. I have given, personally, one hundred baths, some in severe, others in moderately severe, cases, taking as my guide not the temperature, but that *plus* the patient; for in this relation, as in others, patients vary much; the temperature with which one man may be highly delirious, another bears with comparative equanimity. In the administration of our baths, then, we had regard less to the temperature than to the age, the state of heart, restlessness, and sleeplessness, and chiefly to the last. If the patient had not slept for two or three days, even if the temperature fell short of 103°, he was wrapped in a flannel dressing-gown, and placed in a bath of 70° Fahr., gradually reduced to 60° in from ten to fifteen minutes, at the end of which time he was removed to bed, wrapped in a blanket. In nearly all the cases, we thought it prudent to give some stimulant to the patient in the bath, from noting that its effect was to weaken the pulse, which fell but a few beats in frequency, and became thready, compressible, wavered, and sometimes almost disappeared. It required resolution to proceed with the bath in such circumstances—circumstances pointing to impending collapse, a result which occurred in two cases. The general effect of the bath was to lower the temperature, taken *per rectum*, two to three degrees at once, and for about three-quarters of an hour after the bath it continued to fall, about which time the maximum fall had been reached. It then began to rise, reaching its former height in periods varying from one to several hours. In a large number of cases, sleep followed, varying from half an hour to two or three. The general conclusion formed of the value of the bath was that, in the milder cases and in some of the severe cases in the early period of the disease, the bath given once or twice daily was an useful way of relieving the discomfort produced by the heat, of allaying restlessness and producing sleep; that, beyond temporary relief of symptoms, it had no effect on the course of the disease or on the general mortality; and lastly, that in severe cases the remedy was inadmissible, owing to its depressing effect on the body generally, to the exhaustion which its administration entailed; but above all, for its definite and marked effect upon the circulation, which it invariably greatly weakened. In other words, that the bath was useful in relieving symptoms in those cases whose natural termination is recovery; inapplic-

able, by reason chiefly of cardiac weakness, in those cases whose natural termination is death; that is, in the cases in which specific treatment is needed. The depressing effect of the bath has been noticed by Professor Liebermeister, who considers a very high degree of cardiac weakness an important contraindication. What, exactly, may constitute such a condition, will be differently estimated by different persons; but it may be inferred reasonably that, one who has been the subject of a fever whose definite and specific effect is to produce, and that rapidly, some kind of muscular degeneration and death by asthenia; one who has, let us say, passed ten days without sleep and with little or no food, is a person whose condition generally is one of weakness, and whose heart in particular is weak in a very high degree. What might be inferred, a look at the patient shows at once. He lies prostrate, unable to turn on his side or help himself to a drink, the impulse of his heart gone, the first sound inaudible, and the pulse quick and feeble; in short, in a condition of exhaustion. Such a person, I submit, is one in whom there is a very high degree of cardiac weakness. To put the case briefly, those severe cases in which there is every probability of a fatal result from bodily weakness and severity of disease are not cases in which the "antipyretic treatment" is admissible.

It is true that cases such as I have described form but a small proportion of the whole number of cases of enteric fever; but it is also true that they are the only cases in which specific treatment of any sort is required. The great majority of cases recover of themselves, and, as regards their treatment, probably the less of it the better. These cases are cases whose temperatures fluctuate daily between 103° and 104°, rarely remaining any long time at the latter point; and to give them any specific treatment whatever is perfectly useless, for they recover of themselves, and are, in my opinion, best left alone. The remainder which die are cases in which the prominent feature is cardiac weakness, and whose temperatures fluctuate between 104°, 105°, and upwards. These are the cases for which treatment is needed; but I imagine it would occur to no one to give such cases—cases which cannot even be "changed" without suffering from exhaustion—a cold bath every two hours. Liebermeister expressly says that "a very high degree of heart weakness is an important contraindication," and further, that many patients do not bear a very frequent use of the bath. Hence, taking together the cases in which the "antipyretic treatment" is inadmissible from cardiac weakness, and the cases which do not bear it, we exclude from its operation all the cases in which it is desirable that active treatment should be used; and to employ remedies where they are not wanted is, to say the least, a work of supererogation. The "antipyretic

treatment," on the showing of its own supporters, then falls to the ground. The treatment which we have been considering, attracted attention by reason of the very favourable results which were said to be obtained by it. In some cases, the mortality was said to have been reduced to 3 or 4 per cent. Admitting that relapsing fever, the normal mortality of which is about 3 or 4 per cent.; that the "typhus" of young people whose normal mortality is very low; that all febriculæ, in which the mortality is nothing, were excluded; and finally, that the enteric was enteric as we know it in London—points on which by reason of the loose way in which the term "typhus" in Germany is applied to diseases perfectly distinct, I entertain grave doubts—I should still prefer, judging from my experience, to believe that nature of cases, not treatment, determined the favourable results referred to.

"If fever depend upon a poison in the blood, it is not to be dislodged by the act of vomiting, nor washed out by the forcible descent of cold water upon the skin" (Sir T. Watson). I believe the expectant treatment to be the best. There are cases which recover and cases which die; and, in order that we may not prevent the one nor hasten the other, I think the safest course in the present state of our knowledge is to abstain from interference.

An occasional opiate to relieve pain, a little wine to help a failing heart, a laxative to relieve constipation, will be useful adjuvants, if judiciously employed; but I do not think they will be anything more.—*Brit. Med. Jour.*, *Sept.* 23, 1879, *p.* 445.

5.—ON THE PATHOLOGY OF INFECTIVE AND CONTAGIOUS DISEASES.

By Dr. W. S. GREENFIELD, Professor-Superintendent of the Brown Institution, and Assistant-Physician to St. Thomas's Hospital.

At the present moment it seems to me that the relation of lower organisms, especially of bacteria, to contagion, is a topic of especial interest. And, to say the least, there is much confusion in the minds of many as to the bearing of modern discoveries on the life history of bacteria upon the causation and propagation of contagious disease. Perhaps, starting from some elementary facts, I may be able to place some of these problems in a clearer light, and to show what is theory and what ascertained fact.

Moreover, the methods by which these diseases are studied, and should be studied in order to arrive at a sound scientific conclusion, though known to many of you, are, perhaps, not familiar to all; and if I endeavour to place before you some

of the commoner methods of investigation, it may, I hope, render it unnecessary to refer to these methods in future, except in so far as they are modified by experience.

In speaking of the diseases which I have selected, it is impossible to avoid the consideration of the subject of contagion in general, for not only do we continually meet with illustrations of the general laws which govern the transmission and action of contagia, but these diseases furnish us with a starting point for the investigation of the problems involved in the transmission of diseases from one individual to another, and their study is therefore of the greatest importance.

In the practical study of contagion and contagious disease we have to investigate the nature of the contagium, the mode in which it produces the phenomena of disease, and the method in which it is reproduced and conveyed from one individual to another.

We may indeed study the general pathology of contagion apart from the actual nature of the contagious element or virus. We may, for instance, become acquainted with the facts that in a particular disease some product or disease germ, given off in some way from the body, enters another body by a particular channel, after a period of latency of longer or shorter duration, produces the same phenomena as in the previous case, sets up similar changes in the tissues or organs, whether an eruption or morbid growth or some other change, and that again it is reproduced and transmitted from one to another in an indefinite series. We may ascertain the modes of conveyance possible to the poison, the influences which prevent or accelerate its action, the modifications due to individual constitution or to race and species, and the conditions of susceptibility or insusceptibility either of individuals or of classes. In the working out of the morbid process in the body we may be able to trace to some degree the intimate vital reactions on which it depends, and thus to approach more nearly to the discovery of the cause. All the facts thus known may be grouped together as the general pathology of contagion, and may, as I said, be studied for each disease independently, and by the summation of the series certain general laws of contagion may be ascertained.

But we must go further than this—to study the nature of the contagium, and the mode in which it exerts its morbific action, and is reproduced in the body—the intimate pathology of contagion. Nor in this study must we forget the investigation of the phenomena of preservation of the contagious property, external to the body, through long periods of time, or within the body during the period of latency or incubation.

And in relation to all these subjects, let us bear in mind that

we must consider each particular disease apart; that although it may be useful for purposes of hypothesis, and as a guide to experimental study, to make use of analogy, and to make deductions from a number of instances, we must, above all things, avoid considering our analogies and hypotheses as actual facts in dealing with any new case. Each disease and each contagium must be investigated on its own merits.

Contagious diseases, apart from those known to be due to animal and vegetable parasites, may be divided into those which are specifically contagious and those which, though contagious, are variable in their action. I shall speak only of those diseases which are not merely locally contagious, like some skin diseases, or propagations of a particular morbid growth, as tuberculosis appears to be, but are general diseases, such as the zymotic diseases or acute specific fevers, and the contagious blood poisons, such as septicæmia and pyæmia.

It is generally agreed amongst pathologists to separate the true specific contagious diseases from such diseases as pyæmia, septicæmia, and traumatic erysipelas,—from which they are distinguished by several peculiarities. These are, that the specific contagia are not known to arise *de novo;* cases always arise by transmission of the virus from a previous case. The diseases are, within certain limits, "like the father that begat them"—have a similar course, similar symptoms and morbid processes, and reproduce similar cases, which, however modified or lessened by individual proclivity, are always liable to revert to the original type of severity. Moreover, the virus is often specially limited to, or most intense in, some particular tissue or fluid; often that which forms the specific lesion—as the vesicle in vaccinia, the eruption or nodule in glanders,—though not absolutely confined to that part. Many of these, especially those most specific in their peculiar lesion, have, moreover, the peculiarity that, having once affected an individual, they are far less liable to recur than in one who has not suffered from them. At any rate a period of freedom must elapse, which varies in different diseases. Their virus is also usually destroyed by putrescence of the fluid containing it.

The other class, though they may be specifically contagious or inoculable, as some forms of septicæmia, erysipelas, and perhaps diphtheria, can be originated without infection from a previous case, and very commonly arise in this way. They have no period of latency or incubation except such as results from the time needful for entry of the poison into the system, and no specific lesion at all comparable to that of the acute specific diseases. Moreover, they not only usually originate in processes of the nature of decomposition, but their tendency is to cause decomposition of the tissues and fluids, both during

life and rapidly after death. They arise at a certain spot, usually in wounds, and thence infect the system, and hence are distinguished as *infective*. They are usually readily intercommunicable by inoculation to widely different classes of animals, and are not strictly limited to certain classes, as is generally the case with specific infections.

It is clear therefore that, whatever these classes of disease may have in common, they must be regarded as essentially separate, and both their contagium and the mode of its action must be held to be distinct. It is true that we cannot draw an absolute line of demarcation between these classes; for some diseases, such as erysipelas and diphtheria, appear to be on the border-land between, partaking to some extent of the characters of each.

You are all probably convinced that the contagium, in the case of the acute specific fevers, is particulate—that is, consists of definite particles of organic nature,—some of these contagia being capable of long preservation and of conveyance to long distances. But there are two classes of disease in which the evidence as to this particulate nature is less generally recognised: the one, the inoculable diseases, syphilis, hydrophobia, and glanders, and similar diseases known as animal poisons; and the other the large class of diseases which have their apparent origin in conditions of the soil, such as malarial fevers. The latter class does not properly belong to the contagious diseases; but the malarial virus or contagium has so many analogies with these that it is conveniently studied with them. For these diseases, however, there is a very large mass of evidence, into which I will not now enter, that in these cases also the material carriers of the disease consist of solid particles.

Micrococci: Spherical Bacteria.—The simplest forms of bacteria are the spherical or globular bacteria—micrococci. Various names have been given to these: thus one variety has been called *microsporon* by Klebs; and there are other names applied to various groupings of these bodies. They are distinguished from rod-shaped bacteria by their globular or ovoid form. According to Cohn, they are cells, colourless or slightly coloured, which multiply by transverse fission into moniliform filaments consisting of two or more members, so as to form a chain (torula-form); or else into large colonies or balls; or into masses consisting of innumerable members massed together, with but little intercellular substance, which are variously called glœa, zoogloœa (Cohn), or mycoderma (Pasteur). According to Cohn they are motionless. Individual micrococci are usually less than one micromillimetre ($1\mu = \frac{1}{25000}$ in.) in diameter, their size of course varying with the different species, and also to some extent with their nutrition.

Many different terms are used to designate the various forms which the elementary spherules produce by their arrangement—such as chain-form, rosary-form, dumb-bell-form, sarcina-form, and the like. But it is much better to discard these terms, together with names like "leptothrix" and "vibrio," which have no special meaning, merely indicating forms which may be assumed by very different organisms.

The form in which micrococci are usually met with and most readily distinguished in decomposing animal fluids, or in the living fluids of the body under pathological conditions, is that usually called double or dumb-bell micrococci. They have then the appearance of two minute, highly refractile spherules closely joined together; this, in fact, representing an organism in course of division; but such is the activity of reproductive fission that it is the condition in which they commonly appear.

Bacteria proper; Rod-shaped Bacteria.—The second genus, bacterium proper, includes several varieties, of which the most important for our purpose is the common bacterium termo of decomposition. Rod-shaped bacteria consist of short rods which multiply by transverse fission, not forming long chains, but, according to Cohn, only four members at the most being seen in a line. These rods are usually in active movement, having, however, alternate stages of rest and motion at different periods of their existence. In the motile stage they possess flagella, usually one at each extremity, and in all probability this is true of all the forms which possess the power of movement at any stage, though as yet only proved with regard to a few species. In the case of bacterium termo, the flagella were, I believe, first described by Dallinger and Drysdale, and they have since been observed by others. The extreme difficulty in observing them may be judged from the fact that the average length of bacterium termo, as estimated by Dallinger, is only $2{\cdot}5\mu$, or $\frac{1}{10000}$ inch; the diameter of the body $\cdot 8\mu$; whilst the diameter of the flagellum is only $\cdot 08\mu$, or about $\frac{1}{300000}$ of an inch. If, however, a fluid containing bacterium termo in active growth be watched, one may often see during the process of fission that a rod becomes attached to the coverslip, then divides, and the parts separate; but the free rod, which is in more active movement, struggles for some time before it becomes detached, and every movement is attended by a corresponding motion of the fixed rod, showing that though at a distance, the two are still attached by an invisible link, resolved by higher powers into the thread-like flagella.

When bacteria are cultivated they grow into zoogloea masses, in which is a definite permanent intercellular substance,

formed, according to Cohn, by the partial solution of their envelope. The formation of spores in bacterium termo has been observed to occur in two ways. Dr. Ewart (*loc. cit.*) states that he has seen them growing into long rods, in which spherical spores developed at short intervals. These, after two or three days, became free, and lay isolated or formed a zoogloea, subsequently germinating into short rods. The individual spores themselves closely resembled micrococci, but did not, like them, undergo subsequent division. The other form in which spores are observed consists in the presence of a single nucleus-like body at the end of a short rod—a condition not uncommonly seen in putrescent fluids.

It will thus be seen that there are many analogies between the changes occurring in these bacteria and those to be described in bacilli. Bacterium termo comprises members having considerable differences in size and length; the latter usually varies from 2 to 4·5μ. Their especial pathological interest has reference to their constant occurrence in the course of putrefaction, and the experimental researches in the production of sepsin by their action.

3. *Filamentous Bacteria (Faden-bacterien); Bacilli.*—It is in relation to this group, which may be regarded as practically coextensive with bacilli, that the most important discoveries of recent years have been made. And, as there seems to be a sort of glamour about the very name "bacillus," it seems desirable that I should dwell upon the group with some detail.

Bacilli were originally defined by Cohn as consisting of elongated cylindrical parts, which when isolated are similar to bacterium lineola, and multiply by transverse fission; but which also form long chains composed of similar rods, at the junction of which no constrictions are seen. Cohn stated that they often form swarms, but never true zoogloea masses. They, like bacterium termo and lineola, have alternate stages of rest and motion. The facts thus stated by Cohn represent important features of bacilli, but there are many others of no less importance. For we now know that bacilli go through a number of stages of development, and assume various conditions, the formation of spores being one of the most important. In short, bacillus rods may elongate and divide into a number of rods, or assume the shape of long filaments in which spores develop. Again, these spores may become free and escape from the filaments, or the filament may break up into segments corresponding to the spores; the spores may form swarms or remain isolated; and they may, previous to regeneration of the filaments, themselves undergo subdivision. In describing these changes more minutely I shall divide the

genus bacillus into two groups, and shall take as types of each bacilli which I have myself observed.

The first class presents a transition stage from bacterium termo, and it may be a question whether further observation will show its greater analogy with that class, or whether it is a merely accidental variation in mode of growth. It is a class in which the rods, after multiplication, do not form long convoluted filaments, but arrange themselves in heaps, not unlike the zooglœa of bacterium termo. I have been able to watch the growth of only one bacillus of this kind; it appears to be identical with one described by Dr. Koch, as occurring under similar conditions.

The second class, that better known, consists of the group of bacilli which comprises bacillus anthracis. What I shall have to say with regard to the changes seen in bacillus anthracis holds good for the most part of several other bacilli.—*Lancet, Jan.* 17, *and April* 3, 1880, *pp.* 83, 517.

6.—ON THE TREATMENT OF DISEASE.

By Dr. JOHN SYER BRISTOWE, Physician to St. Thomas's Hospital.

No surgeon who only too often finds his skill useless to save, or has to content himself with trimming up or patching the maimed fragments of humanity, can hesitate to acknowledge that the prevention of diseases and injuries has far higher claims to be regarded as a science, and to the gratitude of mankind, than the mere curative or palliative treatment of diseases or injuries which have already accrued. But prevention, in relation to the conditions which determine sickness, injury, and premature death, is a very wide subject.

I am one of those who look with a good deal of scepticism on most that is generally inculcated with regard to the medicinal treatment of disease. I do not for one moment mean to imply that the physician is powerless in the presence of disease; and that he should sit with folded hands while it takes its course unchecked towards recovery or death. I believe that there are many diseases in which remedies, judiciously applied, are of inestimable value; and that there are periods in most diseases when appropriate medicinal treatment is attended with important beneficial results. It is impossible to deny the efficacy of quinine or arsenic in the treatment of ague; of mercury or iodide of potassium in the treatment of syphilis; of colchicum in the treatment of gout; of salicylic acid in the treatment of acute rheumatism; or of iron in the treatment of chlorosis. It is impossible, also, not to recognise the utility of purgatives or diaphoretics in cases of uræmic poisoning; of

digitalis and iron when the heart is acting feebly and irregularly; of opium and its preparations in relieving pain or spasm or procuring sleep; of quinine or other tonics in promoting convalescence, especially from diseases which have wrought debility and emaciation. And the list might be largely increased. But I am satisfied that many affections which we profess to cure, and for the treatment of which definite rules are laid down in books, run their allotted course in spite of all our remedies; and that in a very large proportion of cases all the nicely balanced combinations of drugs which are administered in the form of pills and potions, might, for any good they do, as well be thrown to the dogs. But, gentlemen, although I take what may, perhaps, be an unduly sceptical view of the value of the direct medicinal treatment of diseases, I am, on the other hand, quite satisfied of the immense benefit to be derived from the expectant treatment of diseases, applied with forethought and knowledge. I regard it as one of the chief claims of pathology to the high place it holds among the sciences on which the practice of medicine is built, that the knowledge which it gives of the nature, of the progress, and of the tendency of diseases, and of the various complications to which they are liable—in other words, of the natural history of diseases—enables us to foresee the special dangers to which those who are suffering from particular diseases are exposed, and, foreseeing, to guard against them, to counteract or prevent them. And I am satisfied that, as pathological knowledge advances, our powers in this respect will be largely if not indefinitely increased. The late Dr. Graves, looking back upon the labours of his life, held it to be his chief claim to the gratitude of mankind that "he had fed fevers"; and even, if my memory serves me, intimated a kind of wish that this fact might be inscribed upon his tombstone. But when Dr. Graves fed fevers, and expressed his own sense of the value of his achievement, he was ignorant of the fact that under the head of continued fever were comprised at least three different kinds of fever—each having its special cause, its special pathology, its special dangers, and demanding its special treatment. And certainly, whatever may now be thought in regard to the feeding of typhus and relapsing fever —which constitute two out of the trio—few at the present time would venture to follow Dr. Graves's dictum in the case of the remaining (namely, enteric) fever. Indeed, now that the pathology of enteric fever is understood, and now that it is recognised that the chief causes of its fatality reside in the ulceration of the bowels which attends it, in the persistence of diarrhœa and vomiting, and in the occurrence of hemorrhage or perforation, our chief hopes of successful treatment are made to depend, not on feeding the patient in the sense in which Dr.

Graves employed the term, but in so reducing and regulating his diet as to guard it from irritating the stomach and bowels and thus maintaining sickness and diarrhœa, or inducing either of the other accidents above referred to. I quote this, not for the sake of casting ridicule on Dr. Graves, whom I regard as one of the great lights in medicine, but because enteric fever affords an apt illustration both of the practical value of the progress of pathological science, and of the benefit to be derived in the treatment of disease from an exact knowledge of its tendencies and of the special dangers which attend it. But the remarks which I have made as to enteric fever apply, *mutatis mutandis*, to most other affections. We know that one of the chief dangers to which a scarlatinal patient is exposed is the supervention of nephritis with albuminuria and dropsy; and consequently we take special precautions, and generally with success, either to guard against the occurrence of this complication, or to prevent it from attaining dangerous proportions. We know that when rheumatism attacks young persons it is probable in a very high degree that cardiac inflammation will arise; and consequently, if we have even a very mild case of rheumatism in a child to deal with, we take the utmost care to avert the impending calamity. Again, we know that persons who are suffering from diabetes or Addison's disease are, even at a time when they seem to be in moderately fair health, peculiarly liable on exposure to cold, or after a little unusual fatigue, to fall rapidly into a condition of fatal collapse; and, if wise, we take measures accordingly; and many a life has been prolonged by such precautions, which otherwise would have been prematurely sacrificed.

These remarks, which might be indefinitely extended, apply only to expectant or preventive treatment in relation to the incidents and complications of diseases which have already been contracted, and to the claims which prevention has to be regarded as one of the essential factors in the scientific treatment of disease.

It is the knowledge of the causation of disease which gives us the best clue to the prevention of disease, and enables us, as a rule, to grapple with it most successfully. But the subject of etiology, although one of great interest, is also one of extreme difficulty, and one in respect of which, while much has been done, much more remains to do; and, indeed, much of what seems to be already known is probably rather to be regarded as provisional or approximative than absolutely true. It is a subject, too, in the investigation of which medical officers of health are taking, and ought to take, a leading part. I will very briefly advert to some of the more important or interesting points in connection with this subject and its application to

preventive medicine. That a large number of diseases are inherited, or that a proclivity to them is inherited, is beyond dispute, and that the tendency to transmit such diseases is largely increased when families in whom the same affections prevail amalgamate in marriage, and especially when the members of a diseased family intermarry, is a universally admitted fact. It is in this way that insanity, idiocy, epilepsy, hysteria, and many other nervous disorders, that scrofula in all its various phases, that carcinoma, that gout, that certain skin diseases, that many malformations, and that weaknesses of constitution or defects of mind,—which impart in the former case a readiness to contract certain diseases or to fall into premature decay, and in the latter case tendencies to many of those varieties of foolish or criminal conduct which make persons a cause of continued anxiety to friends, or lead them to the police-court, the prison, or the gallows,—are too often propagated. It is no crime in the father or the mother that such potentialities should reside in their respective reproductive elements; and it seems impossible by legislative enactments to prevent such persons from marrying and pro-creating morbid specimens of humanity. Still, we have reason to hope that as knowledge increases, and as the laws which determine the inherent healthiness or unhealthiness of offspring become more thoroughly understood and appreciated, marriages of consanguinity, and marriages between persons who, on the grounds above given, are manifestly unsuitable for one another, will become more and more rare.

Certain diseases are what are called endemic. They depend on causes which seem to be inherent in certain localities, and to be due to peculiar conditions or combinations of conditions pervading the soil, the water, or the atmosphere of these localities. Among the most typical of these may be included ague or malarious fever, goître and cretinism, and dysentery. Now, it is needless to remind you that although the exact causes of those several maladies are unknown, we know enough, at any rate in regard to some of them, to enable us to deal with them more or less successfully, and so to prevent the prevalence of the maladies to which they give rise.

One of the most important classes of diseases is that which comprises the febrile and other disorders which are communicable from man to man, or from other animals to man, and which severally depend on some specific poisonous agent or virus, which multiplies in the body, and is discharged thence by various routes and in different ways, to plant itself in, and thus infect, other suitable organisms. These are mostly epidemic affections; but they include some of those which have just been referred to as tending at times to present more or less

of an endemic or local character. I shall not stop to enumerate the diseases which I should be inclined to place in this class. But they are among those in which it is presumable that a knowledge of the nature and behaviour of their several specific causes should best enable us to deal with them preventively. They are also among those in regard to which the advance of pathological and sanitary science has effected most in this respect. Of infectious diseases some are imparted only by the direct inoculation of their contagium or specific virus. Of these, hydrophobia, glanders, and syphilis are the most conspicuous examples; and it might be supposed, from the comparatively coarse and visible way in which they are imparted, that they would prove easier to eradicate than probably all other diseases. Yet this is not the case.

There are at least two diseases—namely, Asiatic cholera and enteric fever—which are communicable mainly, if not alone, by means of the alvine evacuations. And it is in regard to our knowledge of the causes and means of prevention of these that recent sanitary science has been most conspicuously successful. When cholera first reached this country, in 1832, it was looked upon as syphilis had been looked upon some centuries ago—as a malady "which," to use Lord Dundreary's expression, "no fellow could understand." How it originated, how it spread, what its nature was, and what were its relations to other diseases, constituted an inscrutable mystery; and consequently numberless modes of treatment, and numberless modes of preventing its spread, were blindly suggested, blindly tried, and believed in with all the credulity of ignorance. In the next and succeeding European epidemics, however, observers of eminence, and among them certain belonging to our own body, conducted a series of researches which have proved, at any rate, that the chief part of the poison is contained in the intestinal discharges; that this poison, little infectious at the time of its escape, attains its chief virulence in the course of three or four days; and that the disease is propagated generally through drinking-water which has become contaminated with these discharges. The importance of these facts in relation to the prevention of cholera has now been demonstrated on a large scale in this country, and there can be no doubt that we have in our present sanitary arrangements a valuable means of keeping cholera epidemics in abeyance whenever hereafter they may unhappily reach our shores. Our knowledge of the causation of enteric fever is probably as accurate as that which we possess in regard to cholera. Within the present century, almost within the present generation, this fever was confounded with several others, and its cause and means of prevention were necessarily alike unknown.

But now nothing is more surely established than that this disease is the product of fæcal poisoning—probably not of the ingestion of healthy fæces, or even of fæces which have undergone more or less decomposition, but of fæces discharged from the bowels of those suffering from the disease. This clue to its causation once suggested, it has been interesting to watch how, of late years (again largely through the labours of the school of sanitarians who have been educated under Mr. Simon's eye, and of medical officers of health), the unsuspected sources of this disease have been tracked to drinking-water directly contaminated with sewage, and to articles of diet which have been mixed or prepared with such water, and especially to milk which the vendors have unrighteously diluted with the contents of their filthy wells. It is needless to add that few diseases are more under the control of sanitary science than this widespread and ever-dangerous affection. Most of the other infectious fevers are transmitted by the breath or by emanations from the surface of the body. As regards these our powers of prevention are, excepting in one or two instances, much less apparent, and I fear actually much less, than in the cases just considered. The main exception is small-pox, which may be prevented by previous vaccination. In most others, however, there are certain peculiarities in regard to the behaviour of the contagia, which become more or less serviceable to us in dealing either with isolated cases or with epidemic outbreaks. It is important to recollect, however, that probably in none of them can we prevent spread excepting by isolation and cleanliness, and that so-called disinfectants have probably no power in destroying their poisons, excepting so much of them as may be imbibed by articles of dress and such-like things which admit of being directly treated by the disinfecting agents. But the difficulty which applies to quarantine, and which induces many of our greatest sanitary authorities to regard quarantine as useless—namely, that there always remains some weak link in the chain of protection, through which the disease escapes—applies also here, and often renders futile all efforts to prevent contagion. It is not surprising that it should be so, even under the most favourable circumstances; it is certainly not surprising, therefore, that that should be our present experience, when the ignorant poor regard many of these diseases, as the over-religious do syphilis, in the light of a visitation from God, which it is impious to oppose; when most persons in whose families they occur are so thoughtless as they generally are in regard to the safety of their neighbours and friends; and when, even now, physicians and medical practitioners—who, one would suppose, ought to know better—continue to regard

the most infectious of all infectious diseases as non-infectious; apparently for no other reason than that the very virulence of their infection causes them to be constantly present in our midst. There are many who even now believe that the poison of neither mumps, nor measles, nor whooping-cough can be carried by the doctor or the nurse from one patient to another; and especially there are many who regard whooping-cough, which is probably the most infectious of diseases, as utterly devoid of infectious properties.—*Medical Times and Gazette, Nov.* 1, 1879, *p.* 493.

7.—ABNORMALLY HIGH TEMPERATURES.

By JOHN W. TEALE, Esq., F.R.C.S., Scarborough.

In February 1875, I brought before the Clinical Society the particulars of a remarkable case which had occurred in my practice. A young lady having received serious spinal injuries from a fall from horseback, had for sixty days a temperature ranging from 108° Fahr. to 122° Fahr. and upwards, and eventually recovered. The great length of time that the temperature lasted, enabled me to take my observations with the utmost deliberation, and the greatest care to guard against error or fraud. A great variety of instruments were used by me, seven in all, exclusive of the one used by my brother, Mr. T. Pridgin Teale, of Leeds, who saw the case three times with me in consultation. Most of these afterwards received the Kew certificate of accuracy, and all were supplied by Harvey and Reynolds of Leeds. Observations were taken at all sorts of times, both o day and night, by myself. More than once in the night I took the temperature when the patient was asleep, having carefully placed the thermometer in the axilla without rousing her. Observations were repeatedly taken in both axillæ simultaneously, once in the rectum, once in the mouth, and once in both axillæ and between the thighs simultaneously. On these special occasions, the temperatures were also taken by myself. Trained nurses, three different ones during the illness, were constantly in attendance upon her, specially cautioned to watch her. The patient was never made aware of the extraordinary character of her case, and for the greater part of the time was too ill to take any interest in the matter. Hot-water bottles were never in contact with the body, except sometimes to the feet by my special order. I was in the habit of carefully removing the instruments into the adjoining room, and the height was read off in the presence of two or more witnesses.

In July of the same year, 1875, I wrote in the Journal a letter replying to various questions and sceptical criticisms on this extraordinary, and at that time unique, case; and I was

able, owing to my patient having had a relapse under the care of another medical man, to give that gentleman's experience of her high temperature. This gentleman, Mr. Hind, of Stockton, told me afterwards that on one occasion the heat in the axilla was so great, that the mercury burst the tube at the top of the instrument, the index being found in the broken-off air-space at the top.

This, so far as I knew, was the first case published bearing on these abnormally high temperatures. It was the first which seriously challenged the then accepted view of physiologists, that such temperatures were incompatible with human life. As long as this case stood alone, it was natural that scientific men, whilst cordially acknowledging the care that had been taken in watching it, should suspend their judgment. I was content to wait, feeling confident that, if my case had been carefully watched and faithfully reported, as I knew it had been, a time would arrive when other cases would be discovered to confirm it. As time went on, first one case and then another was published in the medical journals, all proving the occasional occurrence of temperatures, usually considered to be fatal, in persons who not only recovered, but who, at the time of the excessive temperatures, did not appear to be in imminent danger of dying. This series culminated in that of Dr. Roddick, of Montreal, quoted by Dr. Donkin in his paper in the British Medical Journal for December 20th last, with a recorded temperature of 117° Fahr., in which the mercury was lost in the bulb at the top of the thermometer. The temperature reached 117° Fahr., but how much higher it might have gone cannot be known, as the instrument could not record it.

I claim Dr. Donkin's interesting series of cases as a strong corroboration of the truthfulness of mine. The following points seem now to be clearly established: 1. Temperatures above the degree formerly supposed to be necessarily fatal do sometimes occur without a fatal issue; nay, even without extreme peril to life. 2. Such exceptional and excessive temperatures as a rule end in recovery. 3. The conditions of body in these cases of excessive temperature appear to be distinct from the conditions existing in fevers, in which the rule as to the extreme peril of temperatures of 107° and upwards remains unassailed.

What the explanation of these rare and exceptional cases may be it is for physiologists to work out, but it can never be settled if they shut their eyes to facts which contradict their theories, and relegate such cases to oblivion.—*British Medical Journal, Jan.* 24, 1880, *p.* 126.

8.—ON THE CAUSES OF THE RISE AND FALL OF TEMPERATURE IN ZYMOTIC DISEASE.

By Dr. JOHN SHEA, B.A., Physician to the Royal Berkshire Hospital, &c., Reading.

The chemistry of brewing has much in it that tends to throw light on the fermentive processes that seem to occur in zymotic diseases. The brewer has to deal with a fluid full of organic matter (the wort), very prone to change and decomposition; even a tolerably strong unboiled infusion of malt will, after a few days' standing exposed to the air in a warm place, evolve carbonic acid freely, become acid and turbid, and throw down a sediment which itself acts as a ferment if added to dilute saccharine solutions. But the brewer does not depend on a chance ferment for producing the changes he requires; he is very particular about the ferment he uses, that it shall be as nearly as possible *always the same*, and he dreads the introduction of other or "false ferments." A moderate quantity of sound growing yeast is added to the wort on which the brewer operates, and, at a suitable temperature, the *kind* of fermentation he desires is produced. In the act of fermentation there is a *rise* in temperature, checked, if needed, by the application of cold (by the circulation of cold water within a coil of pipes immersed in the wort), and, as the act of fermentation proceeds and becomes complete, a crop of fresh *new* yeast rises to the surface of the beer, and is removed for further use in fresh brewings. The yeast first employed undoubtedly perishes in exciting fermentation in the wort, but it gives rise to, and developes, a fresh crop of new yeast-cells; and it is one of the brewer's great anxieties that this freshly-developed yeast shall not be inferior to its progenitor, but shall be a healthy, strong, and active ferment, capable of again producing the *same kind* of fermentation as before. It sometimes happens, however, that by some mismanagement in the brewery the yeast gets deteriorated, becomes at each brewing weaker, and at length perishes; or it becomes charged with forms foreign to healthy yeast, and does not excite the proper kind of fermentation. The brewer then abandons this yeast, and seeks a fresh supply elsewhere, often from some neighbour. Presuming, however, that the process has gone on successfully to the alcoholic stage, it is then the aim of the brewer to check any further fermentation; the beer has already been "hopped," and the bitter of the hop acts as an *anti-ferment*, preventing further change, but it is extremely needful to let all the yeast "work out" of the beer before the barrel is finally bunged up, or the beverage becomes what is called "yeast-bitten," and very liable to acetous fermentation.

It is by no means difficult to draw an analogy between the process of brewing and the process that goes on in the human body affected by zymotic disease. In zymotic disease there is (1) an organic fluid—the blood—susceptible of change; (2) this fluid may be fermented by the introduction of disease germs which act as ferments; (3) the fermentive process causes a rise of temperature; and (4) the kind of fermentation will depend on the kind of ferment or germ introduced, just as the quality of yeast will govern the sort of fermentation set up in the brewery; (5) the act of fermentation being accomplished, the mature form of the ferment disappears or perishes, but in so doing gives rise to a fresh crop of spores, which, *after an interval*, re-develope the mature form of ferment, and with it the fermentive process and a consequent further rise of temperature; (6) this sequence can be checked, more or less completely, by the application of cold, or by the administration of some bitter, such as quinine, which acts as an anti-ferment, just as the bitter of the hop acted in the beer.

The analogy ceases here, for the brewer is anxious to preserve his ferment for fresh brewings, whereas the physician desires that the ferment in the blood of a fever patient shall perish, lest by repeated fermentation it exhaust both the nutritive power of the blood and destroy the nervous power in the system. Where the fermentation is not severe, or the natural powers of the patient in resisting exhaustion are good, a time comes when the ferment itself perishes. The death of the ferment may be aided by cold (as baths) or by the administration of anti-fermentive remedies like the cinchona alkaloids or salicylic acid; and it is found that a *full* dose of quinine given shortly *before* the expected rise of temperature acts best, probably in retarding the further development and regeneration of the ferment. The analogy between the effect of disease-germs on the blood and the action of yeast in wort is by no means new. Liebig has drawn the same analogy.

The sudden excessive rise of temperature in such diseases probably depends on, or is concurrent with, a development of the *mature* form of an exciting ferment, and the researches respecting the condition of the blood in "relapsing fever" seem to prove this. At the height of that fever the blood is found full of moving rods ("spirillæ"), which disappear as the temperature falls, and are again found if the temperature again rises. There can be little doubt that in the interval between the two rises of temperature, though the mature rod-like forms of the spirillæ cannot be observed, the spores are present as minute micrococci-like bodies. In zymotic diseases, if the fermentive action is very violent, the temperature rises very high, and a crisis, or "perturbatio critica," may occur,

followed by an *abnormal fall* of temperature, due to the exhaustion of the vital powers, consumption of tissues, and deterioration of the blood, induced by the high temperature, or rather its cause. A tendency to collapse then occurs, and the lowered temperature and impoverishment of the nutritive character of the blood itself may bring about the *death of the ferment* and cessation of the fever, or even the death of the sick person himself, if the fermentive changes have been so violent that the essential functions of life are interfered with from the exhaustion of nerve-power. Thus, after or at such a "*crisis*," the blood will supply less pabulum for the re-development of the ferment, which then perishes, and with it ceases the fermentation of the fluids of the body, and the temperature resumes its normal state. But, presuming that the ferment does not perish and the patient still survives, the abnormal fluctuations of temperature will continue (1) as long as the ferment itself finds material to feed upon from which it can re-develope itself, and (2) so long as the blood and fluids of the body are duly renewed by assimilated food, and such blood and fluids retain the capacity of supplying nutriment to the ferment, and of supporting the fermentive process. The existence of effete matters in the blood may greatly help to support the ferment, and their removal help in its extinction. It can scarcely be doubted that in a *healthy human* body, where the effete matter is small in quantity, there is a *resisting influence* to zymotic disease, due also possibly to the presence of some natural anti-ferment in the blood—"animal quinoidine"—and to the absence of a pabulum on which the diseased germs can feed most readily.

The rise of temperature in fever is attended with the development of the mature form of the exciting ferment, which in its turn gives birth to fresh spores. If then the ferment can be killed, or the fermentive process checked *before this rise again occurs*, a stop will be put to the re-development of fresh spores or disease germs. In the intervals *between* the rises of temperature in certain diseased states, although the mature form of the ferment present has not been found, its place has been occupied by bright refractive micrococci-like bodies from which it can be re-developed. From the study of the life-history of these minute organisms it is known that the mature forms will perish at a temperature of 140° F., whilst the bright nuclei they contain resist heat of 260° F. The germs, therefore, are far more vital than the mature forms of these ferments, and are more difficult to kill. These germs may lie dormant in the viscera or lymphatics till circumstances favour their re-development or further activity. It is in this way only that the long intervals that sometimes occur between the attacks in

ague can be explained; and the benefit that arises from the exhibition of a full dose of quinine before the next fermentive act or attack occurs is well known. Ague is a disease in which the periodicity seems clearly to mark distinct recurrent processes of fermentation (the theory propounded, that the disease is merely due to "chill," seems perfectly untenable), and there can be but little doubt that although Dr. Salisbury may have misnamed the algæ he found in malarious districts—confounding it with a harmless form, the "palmellæ"—he is correct in attributing ague to the action of a minute vegetable organism on the blood. The more recent researches of Klebs point entirely in the same direction, and it is now confidently asserted that the ague poison can be collected and concentrated, being a minute alga—the Bacillus malariæ. Cohn considers all the bacteria to be *algæ;* and if one form will produce ague, other forms may each produce specific diseases. Not only does quinine cut short the hyperpyrexia in ague, but the early administration of a *large* dose of this anti-ferment is well known to reduce the temperature in enteric fever or pythogenic pneumonia. Salicylic acid also acts as a blood anti-ferment in rheumatic fever of malarial origin, and the same compound is somewhat largely used to preserve organic fluids in daily use. Although a fall of temperature usually follows the administration of a *large dose* of quinine, this result is not always seen, and the exhibition of powerful doses of anti-ferments in zymotic diseases affords *no certainty* of a favourable issue to the case. It may be that the anti-ferment is not given soon enough or in a sufficiently large dose to counteract the ferment introduced into the body by contagion. The medicine itself may tend to "slow" the pulse and still further depress the vital powers; or the nervous system may at the very outset have received too severe a blow from the blood-poisoning to recover its power; or lastly, the blood itself may become disintegrated from the violence of the action set up by the ferment absorbed, especially if the amount absorbed be large.

Disintegration of the blood is not uncommon in malarial blood-poisoning, where the dose of the poison and its results are both excessive. Where only a moderate dose is absorbed, the blood may sustain many repetitions of the fermentive act, may be deprived of much of its nutritive principles and salts, and yet not become disintegrated to such a degree as to produce purpura. The impoverishment of the blood in saline matter may be due to action of the ferment on the blood. It is a known fact that certain bacteroid forms seem to flourish in and live upon distilled water charged only with certain salts; and the use of saline medicines in fevers may be due to the counteraction of this impoverishment, and the combination of salines

with an anti-ferment like quinine is often found to be useful. The rise of temperature is rarely so excessive in elderly persons as in young adults. After forty years of age very high temperatures are seldom observed in cases of enteric fever. Where the person affected is over forty the body has also, in all probability, sustained already several acts of fermentation from previous diseases, such as scarlet fever, etc., and is less prone than in youth to take on the fermentive process.

Although the existence of "special ferments" is widely denied or questioned, and objections are raised to the fermentive theory of zymotic diseases, the abnormal *rise* and fall of temperature are not easily explained on other basis. Vaccine lymph is said to be true infective matter destitute of bacteroid forms, but it is on good evidence stated to be full of micrococci-like bodies. Pyæmia also is said to depend on a poison ("pyrogen") which does *not multiply* in the body, but acts according to the dose absorbed; but it is admitted that in *nearly all* cases of pyæmia the blood has been found to contain various forms of bacteria, and where the *mature* forms were not found, it is by no means certain that their *germs* were absent. In certain diseases the germs may play a more important part than the mature forms, and, by gaining access to the *soft nerve-centres*, cause intense depression of nerve-power, such as is seen in diphtheria. Till some more probable theory arises, the fermentive theory in zymotic disease appears the most possible.—*Medical Times and Gazette, Dec.* 20, 1879, *p.* 687.

9.—ON THE EXPERIMENTAL STUDY OF DISINFECTANTS.

By Dr. J. Lane Notter, B.A., M.Ch., Surgeon-Major, Army Medical Department.

The term "disinfectant" must be understood only to apply to agents which destroy, by chemical action, not only the decompositions which are causally connected with the presence of organisms, but the organisms themselves.

It is only under such a condition of things that we can say with any degree of certainty that the contagia of communicable disease are deprived of their specific action, and anything short of this will not, in the true sense of the term, be "a destructive agent."

It appears to me desirable, for many reasons, to extend our inquiries into this very complex subject, and more especially to investigate into the power various agents are said to possess not only for correcting the putrefactive odour in offensive liquids, but also in subjugating the development of bacteria.

It will be understood that in the following remarks I do not wish to attach too much importance to the coincident appear-

ance or occurrence of microscopic forms, as if to imply that these were the morbific agents. My object is simply to compare the relative powers of various disinfecting agents, and I believe this to be the very best method to adopt in order to arrive at a correct conclusion.

Hitherto the majority of experimenters have contented themselves with noting the power certain agents possess of controlling offensive odours, and with the sense of smell alone decided the question; but we know as a fact that only a very limited relationship exists between the subjugation of bacteria and the destruction of the putrefactive odour. For example, Condy's Fluid destroys the odour, and the bacteria not only retain their activity, but appear to grow and flourish; while Chloralum, which does not destroy the odour, partly precipitates the bacteria.

In the following experiments I have attempted to place on record a detailed and minute microscopic examination of the effect produced on a measured quantity of beef infusion, swarming with bacteria, by adding to it certain known proportions of various disinfecting fluids and powders.

I believe the full effect only of the agent to be produced when there is an arrest of motion, with complete precipitation and disorganisation of the bacteria, and I have endeavoured in each case to look for this result. I do not consider the formation of bacteria in filmy masses the same as being precipitated. The two forms are quite distinct. In the latter there is total disorganisation; in the former there is only an arrested movement, due to the nature of the surrounding frond in which they are enveloped—they are, in fact, bacteria in the zooglœa form, their nutritive and excretory processes, as well as the development of the putrefactive odour, being as vigorously carried on as when they exist in a free state.

When these organisms rush across the field, I have called the movement *excursive*, and by this will be understood freedom and activity.

The agents submitted to experiment were the following:—1. Carbolic Acid (crystals). 2. Carbolic Acid. 3. Chloride of Lime. 4. Chloralum Liquid. 5. Condy's Fluid. 6. Universal Disinfecting Powder. 7. Terebene Liquid. 8. Terebene Powder (green). 9. Terebene Powder (yellow). 10. Sanitas. 11. M'Dougall's Disinfecting Liquid. 12. Phenyle. 13. Burnett's Fluid. 14. Eureka. 15. Sporokton.

[Dr. Notter, after detailing an interesting series of experiments says:]

The facts ascertained may be thus simply stated in relation to the agents experimented with:—

1. Chloride of lime destroyed the putrefactive odour and the bacteria themselves, no free bacteria being visible.
2. Burnett's fluid acted similarly, but a very slight odour remained.
3. Carbolic acid and Phenyle subdued the offensive odour, while the activity of the quite free bacteria is persistent though diminished.
4. Sporokton slightly subdued the odour and diminished the activity of some of the bacteria, while others maintained their power of motion.
5. The Terebene preparations destroyed the odour and precipitated the bacteria in flaky masses, but left some free, isolated, and almost motionless ones in the field.
6. Sanitas acted in the same way, but the activity of the bacteria suffered less change.
7. Permanganate of potash presented similar microscopic characters, but the bacteria seemed to elongate, and torulæ were developed.
8. Very similar characters were presented by Chloralum, the Universal Disinfecting Powder, M'Dougall's Disinfecting Liquid—the odour being affected to a very small extent, while the activity of the bacteria, though very slightly diminished, is persistent in the interspaces—while some are precipitated, others appear in the zooglœa form.
9. Eureka neither corrected the odour nor checked the activity of the bacteria to any degree worthy of special comment.

If the total destruction of bacteria and their reproductive energy be regarded as the most cogent evidence of disinfecting power, in accordance with our present attainments in this difficult inquiry, the agents above noted are given in the order of their importance. On the other hand, while under ordinary circumstances the impressions of the olfactory sense awaken the apprehension of danger, some of the most virulent morbific agents or influences are so subtle as to be quite imperceptible to that faculty.

On reviewing this series of experiments we find that the correction of the offensive smell and the suppression or destruction of the bacteria do not hold that close correlation that might be supposed *à priori*. But here is, in truth, that nice line of demarcation between Deodorisation and Disinfection—the latter dealing with the cause, and the former with the effect; and until it can be shown that mere deodorants, so called, do not at the same time correct morbific agents other than corporeal bacteria, we do well to be guided by the premonition of the sense of smell.

As the Chloride of Lime has taken first place in these experiments—it being quite unnecessary to add more of the solution to bring about the required result—I would observe that this agent would be more effective than it is commonly found to be if it were previously mixed with water, and then used when well shaken up, after the manner of other liquid disinfectants, instead of being rudely thrown about in the dry state, or mixed up with a little water in a saucer. In these experiments it was found that a solution containing 140 grains of chloride of lime to the gallon was strong enough to disinfect a fluid such as that used in the experiments above detailed.—*Dublin Journal of Medical Science, Sept.*, 1879, *p.* 196.

10.—DISINFECTION BY ACIDS.

By Dr. JOHN DOUGALL, F.F.P.S.Glasgow, Lecturer on Materia Medica in the Glasgow Royal Infirmary School of Medicine, &c.

Acids have an action on putrescible fluids which seems to me of much value in a sanitary sense. I have already referred to this in my paper on the Science of Disinfection (Trans. Social Science Congress, Glasgow, 1874), and the numerous experiments by which it was discovered are detailed in my paper on Putrefiers and Antiseptics (Glasgow Med. Jour., Nov. 1872, and Feb. 1873). A few remarks on this point may not be out of place. If a clear, fresh, putrescible fluid be set aside for a short time, and then examined by the microscope, it will be found swarming with bacteria and their allies in active motion, fœtid, and opaque. Its reaction is neutral; but these changes would ensue though it were made moderately alkaline or *very faintly* acid. If it be now exposed in a cool dark place for from nine to twelve months, and then examined, it will still be found swarming with life, cloudy, fœtid, and containing albumen. In a few months later, putrefactive reduction is completed. The solution may be still cloudy, but is now almost free from fœtor; a bacterium is only seen here and there; a faint trace of albumen may or may not be detected; and its specific gravity, at first perhaps about 1.6, is now probably about 1.1 or 1.2. Such are the chief physical phenomena of putrefaction. As regards its chemical aspects, the chief elements in animal matter are oxygen, hydrogen, carbon, nitrogen, phosphorus, and sulphur. The hydrogen unites with the oxygen to form water; with the nitrogen to form ammonia; with the sulphur, phosphorus, and carbon, to form sulphuretted, phosphoretted, and carburetted hydrogen gases. The oxygen also unites with the carbon to form carbonic anhydride. In course of time, the ammonia is oxidised into nitrous and nitric acids and water; the sulphu-

retted hydrogen into sulphurous and sulphuric acids and water; the phosphoretted hydrogen into phosphoric acid and water. These free acids then at once combine with any bases present to form their respective salts. Putrefaction is thus seen to be a process of reduction; and the greater the number of equivalents of elements forming the atoms of a compound, the more easily does such a compound decompose. This instability is increased in animal substances by their containing nitrogen, which has less tenacity in its affinities than any of the elements. The large quantity of water present also forms a most favourable medium for putrefaction, which only takes place at death, as then the chemical forces by which the living tissues act on vital organic compounds cease, and the elements of the original compounds unite to form bodies less complex, but more stable, as described above.

If to another portion of the same clear, fresh, putrescible fluid, a quantity of acid, mineral or vegetable, be added, so as to cause marked acidity, and the mixture be then placed under the same conditions as the former, then in a day or two it shows quite a new set of changes. One or more small tufts of mycelia are present, their silky filaments radiating from a tiny nucleus; also numerous free spores and double torulaceous cells. If the proper quantity of acid have been added, no bacteria are present, and the fluid, instead of becoming hazy, becomes clearer and sparkling. Its reaction is of course distinctly acid, its odour somewhat mouldy, but not at all unpleasant; while, in about four months, it ceases to respond to albumen tests, indicating that decomposition is completed. This is fermentation. Again, if to a portion of a putrefying fluid a slight excess of acid be added, putrefaction is soon arrested, and the mixture, instead of being then fetid, has an odour like that of oil of spiræa. And if to separate portions of a fresh and of a fermenting albuminous fluid, a larger quantity of acid be added than is sufficient to cause fermentation alone, then the fresh fluid neither putrefies nor ferments; fermentation is arrested in the fermenting fluid; and both mixtures remain in this antiseptic state for an indefinite time, and latterly cease to respond to albumen tests.

Now, these results seem to me strong proofs that putrefaction and fermentation, whatever be the chemical changes they produce on solutions of animal fluids, are distinct processes. There is abundant evidence that putrefying matter is very injurious to health, but there is little or none to show the effects of fermenting matter in this respect. It is obvious, however, that fermentation must be harmless as compared with putrescence. Putrid matter evolving noxious effluvia for nearly twelve months must be more hurtful than the same matter

when fermenting, it being always almost odourless and fully decomposed in about four months. Moreover, as already observed, acids added in excess both prevent and arrest putrefaction and fermentation; and this not by antiseption but by destruction, as is shown by the animal solution ultimately ceasing to respond to the tests for albumen, without the physical signs of either putrefaction or fermentation having been present.

Convinced of the truth of these facts and deductions, I have now for a number of years exclusively practised them in every form of infectious disease, for which there has been ample opportunity both in my capacity as medical officer of health and in private practice. I have watched the effects with scrupulous care and an unprejudiced desire to learn whether, and if so how far, theory and laboratory experiments were confirmed by practice. My experience and observations have further convinced me that disinfection by acids is cheap, quick, easy, and certain; and I have frequently been struck with the summary manner in which an infectious disease has been thus arrested, under circumstances most favourable for its propagation.

About eight months ago, I attended a girl, aged 6, suffering from scarlatina. The family—besides the father, mother, and a servant girl—consisted of eight children, four older and three younger than my patient, none of whom had the disease. The house contained four small rooms and kitchen. The girl was kept in a room by herself; none of the family were allowed to see her; but during her illness, which lasted about a month, they remained in the house. The mother nursed the sick girl, at the same time mixing freely with the other children, four of whom had to stay from school, thus greatly increasing their risk of being infected; but as yet none of them have taken the disease.

About five months ago, I attended a male, aged about 18, son of a sanitary inspector. I found him very prostrate, and covered with a livid scarlatinal rash. Next day, he was greatly worse, and the case looked hopeless. The house consisted of two rooms and kitchen, and contained, besides the father and mother, four children, excluding my patient, whose ages ranged from six to sixteen, none of whom had the disease. Two days afterwards, a brother of the patient turned ill and was fevered. It was assumed that he was also sickening with scarlatina, and hence was put in a bed in the same room with the patient; but recovering in a couple of days, he was removed to a friend's house. Briefly, my patient, after a rather protracted illness, with profuse desquamation of the cuticle, quite recovered. The other children lived in the house, and as

usual were kept from school. The mother nursed the patient, mixing freely with the rest of the family; and yet, up to the present, none of them have taken the disease.

About two years ago, I attended a girl, aged 12, ill of typhoid fever. The house consisted of four rooms and kitchen. Besides the father, mother, two servant girls, a sick nurse, and my patient, there were five young children staying in the house. The patient's bowels were very loose for about three weeks. She ultimately recovered; the children were kept at home during her illness; none in the house previously had, nor subsequently took, the disease.

About four years ago, I was hurriedly called to see a boy, about six years old. I found him in a state of collapse, and covered with a petechial scarlatinal rash; he died next day. The house consisted of two rooms and kitchen; besides the father, mother, and my patient, other four children stayed in the house, none of whom were sent away, nor had the disease previously, nor were affected soon afterwards. About twelve months subsequently, however, the family still residing in the same house, and the mother having had another child, I was called to see a boy of the same family, aged about five years. He was covered with a bright scarlatinal rash, and ultimately had slight albuminuria during profuse desquamation, but made a fair recovery; the domestic arrangements were the same as with the first case. None of the rest of the family were affected.

I could relate a great number of similar cases, but that is unnecessary, as it seems to me obvious, from the highly favourable conditions, surrounding those detailed, for the spread of infectious disease, that its arrestment must be greatly, if not solely, attributed to the means of disinfection employed. I do not, however, wish it to be understood that I have been so successful in all my cases of infectious disease, the subtlety, attenuation, and extremely obscure modes of operation of zymotic poisons being such as frequently to baffle most, if not all, attempts to modify or prevent their action. Again, it is next to impossible to get some people to bother themselves in carrying out systematic instructions as to disinfection; they do it either spasmodically or imperfectly. In these latter cases, I usually find the results much more unfavourable than where the instructions are faithfully attended to. On the other hand, so far as I can remember, every focus of infection under my care, the spread of which had been so suddenly arrested under conditions most favourable for its transmission as to impress it on the memory, was disinfected entirely to, and sometimes, I thought, beyond, my satisfaction. Moreover, every year of increased experience confirms me in these views.

For disinfecting, I advise the use of three acids—hydrochloric, sulphurous and aromatic glacial acetic. The hydrochloric consists of one of the strong acid to twenty of water, and is used chiefly to disinfect typhoid excreta, and the bed and body clothes of persons with infectious disease. In typhoid cases, a large cupful of this fluid is put in the vessel before it is used. After being emptied down the watercloset, it is rinsed with another cupful, which is also at once put down the closet, the water running while this is being done. In using this fluid for clothes, these only require to be sprinkled with it, so as to sensibly damp them, allowing them to remain so for about four hours; but of course saturation for about an hour is preferable. With thick woollen clothes, such as blankets, however, an inconveniently large quantity of the acid fluid is needed for saturation. It is worth remembering, in such cases, that wool resists the action of acids far more than cotton or linen, and as this material is more likely, from its porous nature, to retain infection than the latter, it should be subjected longer to the action of the acid. Boiling water is afterwards poured over the acidified clothes, which are allowed to stand for an hour. They are then thoroughly rinsed with warm water till all the acid is got rid of, when soap with soda or potash may be used to wash them. If the acid be not removed, the soap will not form a lather. The solution of this acid may also be used for sprinkling on floors, pouring down sinks, etc.

The sulphurous acid is used exclusively for aërial disinfection, which is done morning and night during the patient's illness, not only in the patient's apartment, but in every room in the house. The process is simple; a piece of live coal is placed on a shovel and powdered sulphur sprinkled on it. The sulphurous fumes thus generated should be made to impregnate the entire atmosphere of the house, until it becomes somewhat irritable. If more fumes have been generated than can be tolerated, the excess is easily got rid of by opening the doors and windows, and by allowing what may be still forming to ascend the chimney. Fifteen minutes is sufficient to allow the acid to act each time it is used; but if the apartment be empty, it may be used some hours. Mattresses and carpets can be disinfected with sulphur fumes by suspending them spread out by the four corners with ropes, and allowing the acid to float thickly along the lower surface for ten minutes, then turning them upside down, and repeating the process. Bright metallic surfaces are darkened by sulphurous acid.

The aromatic glacial acetic acid, though more than a hundred times the price of the others, gives great satisfaction in the using, its well known grateful cooling odour forming a pleasant contrast to the repulsive emanations from the body of an infec-

tious patient. It consists, besides the acid, of the oils of rosemary, neroli, cinnamon, cloves, bergamot, lavender, and rectified spirit. I frequently use this acid as an occasional aërial disinfectant, but never to the exclusion of sulphurous fumes. It is directed to be dropped on a hot shovel and volatilised through the house daily, which is the more effective way; or, where its price is a consideration, a little may be poured occasionally into a small vessel of hot water, placed over the gas, in the patient's room, by which means it becomes more gradually dissipated, producing at the same time a constant pleasant odour in the apartment. I have had much satisfaction also in using it dilute—one drachm to one ounce of water—for sponging the body, by which two objects are attained—lowering of the temperature and disinfection of the skin. Almost without exception, patients like this process, and are usually much refreshed by it. In all scarlatinal cases, more especially during desquamation, I invariably direct this to be done twice daily. The practice, so much in vogue, of smearing the skin of a scarlatinal patient during desquamation with lard or warm suet, to prevent the morbific epithelial *débris* from scattering, seems to me highly calculated to injure the sufferer. By this means the mouths of the sebaceous and sudoriparous glands, the very channels through which, in all probability, the poison finds egress from the blood, are plastered up; so that, besides the unpleasantness it causes, it is an effectual way of sealing the infective matter in the body. It looks precisely analogous to hermetically closing every door, window, crack, and crevice in a room where a person is lying soaked with infection. On the other hand, by sponging the skin with the aromatic acid, the infectious scurf is permanently deprived of its dangerous properties, while, at the same time, the numerous orifices of the gland-ducts are cleared of their morbid secretions, thus giving free exit to the poison from the blood. Moreover, it is well known, and has been proved experimentally, that the evaporation and oxidation of most perfume-yielding oils, including those in aromatic acetic acid, generate ozone—a powerful disinfectant. Here, then, is another strong recommendation for sponging the body with the substance in question, as by this means an atmosphere of ozone may actually be made to evolve gradually and constantly from the patient's skin, floating under the bedclothes and enveloping every part of the body.

It has been urged, as a fatal objection to the use of acids in disinfecting enteric excreta, unsupported, however, by any experimental or practical data, that they corrode and destroy the metallic fittings of the water-closet. That this objection, applied to hydrochloric acid, is for all practical purposes, quite groundless, I have proven hundreds of times; hence it seems to me entirely superfluous to give examples to confute it. One,

however, may be cited. A few years since, during an epidemic of typhoid, I attended a young man ill, and who at last died, of the disease. His bowels were very loose, moving more or less about ten times in twenty-four hours for nearly three weeks. During this period about three-quarters of a gallon of strong hydrochloric acid, diluted in the proportion of one of acid to twenty of water, had been used to disinfect his excretions, all of which was put down the water-closet without affecting the fittings in the least perceptible degree.

In order, however, to obtain some exact results on this point, the following experiments were made with the metals used in closet-fittings. Pieces of brass, of copper, of lead, and of cast-iron, each measuring two inches in superficies, were weighed and immersed in two ounces of a liquid consisting of one part of hydrochloric acid in nineteen of water, for twenty-five hours. After this, they were washed in pure water and again weighed, when the weight of the brass, copper, and lead, was found unaltered, while the iron had lost thirteen grains, or 4·33 per cent. of its weight.

I submit that these results are a sufficient answer to those who aver that closet-fittings are injured by at least hydrochloric acid, the substance I prefer, as it is by far the cheapest of the mineral acids. In putting the acid fluid down the closet, the first metal it touches is the copper pan, containing usually several ounces of water, by which it is at once more diluted; then, when the handle is raised, there is a rush of water, causing a further great dilution; and the fluid is next carried into a cast-iron basin, thence through a leaden trap, into an iron or glazed sewer-pipe. The lever and other movable apparatus of the closet are of iron and brass; and they, being situated externally to the pan and basin, the acid fluid does not touch them at all. Now, as iron was the only metal affected of the four used in the experiment, hence it is the only one likely to be affected in the closet; but the iron pan is about a quarter of an inch thick and the soil-pipe about one-eighth, while the acid fluid as it passes over the iron basin is diluted to at least 1 in 100, and when it goes down the iron soil-pipe to at least 1 in 500; moreover, the time of contact is but a few seconds. Hence it is obvious, considering that the iron in the experiment was in contact with the strong acid fluid for twenty-five hours, that the action of this fluid on the iron of a water-closet must be extremely little. Even supposing the strong acid fluid were to act *per se* on the iron of a closet, say for ten seconds ten times daily during three weeks, as might be required in a bad case of typhoid, and as was in the case I mentioned; yet, on the data of the experiment with the iron, it would take nine hundred days, or about two years and a half, at this rate to dissolve six-and-a-half grains off each square inch of surface of the iron.

In conclusion, it may be stated, as a summary of the foregoing, that putrefaction may be prevented, neutralised, or arrested, or its odour masked or neutralised, and yet any infective matter present, be, in all probability, unaffected, preserved, or only made dormant for a brief period, as exhaustive experiments with carbolic acid and infective fluids, more especially with vaccine, show: that to use purely antiseptic substances as anti-zymotics is highly paradoxical, preservation being practised and destruction anticipated; further, that the mineral acids are true disinfectants, operating by both physical and chemical destruction, and also by transformation, of organic molecules, as in fermentation; hence, they prevent and arrest putrefaction, they transform putrefaction into fermentation; they deodorise; and, moreover, they are highly anti-zymotic as regards vaccine, virus of glanders, virus of infective inflammation, and therefore, by inference, as regards all other infections.—*British Medical Journal, Nov.* 15, 1879, *p.* 770.

11.—CLINICAL LECTURE ON A CASE OF PYÆMIA, WITH VALVULAR DISEASE OF THE HEART.

By Dr. HENRY THOMPSON, Consulting Physician to the Middlesex Hospital.

All are familiar with the chief clinical features of pyæmia as they present themselves in the domain of surgery, where they are far better pronounced than ever they are in the range of medical experience. To the surgeon, rigors are of essential value and significance in the diagnosis of pyæmia. Without rigors he would at once look upon such diagnosis with the gravest misgivings—perhaps he would set it aside altogether as unworthy of acceptance. You must, however, bear in mind that to the surgeon belongs the privilege of witnessing and watching the majority of cases from the onset; many of the cases, indeed, are his own handiwork—the outcome of his own operations. Now, if ever there be a rigor at any time, it is sure to occur at the very outbreak of the malady, and the surgeon is sure to see it or to hear of it. If he does not, he has strong *primâ facie* grounds for disbelieving in the existence of pyæmia. It is otherwise with the physician. Again and again the cases that fall under his charge are at least of some short standing, and the rigors may have come and gone unrecorded and unremembered, or they may have never existed at all. Recollect this, and in medical cases never allow yourselves to be carried away by the foregone conclusion that there is no pyæmia because there is no *history* of shivering. Recollect, moreover, that rigors are really far oftener absent in the

sphere of medicine than in the sphere of surgery, although they are the rule in both departments alike.

The symptoms of pyæmia are, in brief, those of enteric or typhus fever, with a large but varying intermixture of the hectic form. The main distinctive criteria are the following:—If the rigors and the perspirations which commonly signalise the onset are repeated throughout the career of the case in disorderly fashion, you may safely exclude enteric fever, typhus, ague, and acute rheumatism. Moreover, in the first-mentioned the fever-spots are pathognomonic and cannot be mistaken for the sudamina and pustules of pyæmia. Again, the pleuro-pneumonia which may often enough attack the sufferer from rheumatism, and attack him severely too, is seldom so severe, so widely diffused, and so rebellious, as in pyæmia. Even the appearances around the joints, which make rheumatism and pyæmia look so like each other, may, for the most part, be discriminated with care. In rheumatism the redness is faint by comparison, and fades away insensibly into the normal hue of the surrounding skin; in pyæmia it is vivid and crimson-coloured, sharp in its definition and jagged at the margins. Such, at least, is the teaching of my own experience. Again, in the majority of cases of pyæmia there is an earthy-yellow tinge of the complexion, which in many instances may deepen into genuine jaundice. In rheumatism true jaundice is altogether exceptional, although there may be a trace of the characteristic tint in the eye and in the urine. In the common forms of fever, jaundice is so rare as to claim especial recognition whenever it chances to co-exist. Far, however, above and beyond all other criteria is the testimony of the thermometer. The free and fitful oscillations on the chart are unerring and all-sufficient guides to the diagnosis. Nevertheless, it would be well to pause awhile and to enlarge upon the differential diagnosis of pyæmia and acute rheumatism. No two diseases are so often confounded; indeed, at the first sight the confusion is almost inevitable unless there is something in the circumstances of the case positively to foreshadow the coming of pyæmia. I have made the mistake more than once for a moment, or for a longer though limited period; it would be unpardonable to remain in uncertainty or in error to the last. A few months ago I lectured on cerebral rheumatism—a form or manifestation of rheumatic fever which is most nearly akin to pyæmia,—and this for two reasons: it is always accompanied by head-symptoms, as its name denotes, and is often accompanied by hyperpyrexia, or at least by a high degree of pyrexia. In that lecture I besought you to stand on your guard, and to look ahead for the tokens of forthcoming mischief. I warned you then in all cases of acute rheumatism,

and I warn you now in all cases that bear the least resemblance to acute rheumatism, when associated with nervous derangement, never on any account to allow the temperature to go beyond 104°, or at most 105°, before you resort to the bath. I acted on this principle in the present case; I assumed it virtually to be cerebral rheumatism, as I was bound in conscience to do while as yet there was the shadow of a chance that it might accord with my assumption. In so doing, of course I interfere with the natural changes on the thermometer. I do not give the instrument fair play and free scope for the delineation of the disease if, the moment it stands at 104° or 105°, I cool down the body-heat in the bath and reduce it in the end to the normal average, or below that level. This is true as far as it goes; the bath defaces for awhile, but it does not destroy, the distinctive readings on the thermometer—if you read between the lines; in other words, if you look at the lesser elevations, which are untouched by the bath, and disregard the main peaks, which alone are lowered by it. In well-developed cerebral rheumatism with high fever, the temperature will seldom fall spontaneously more than a degree or so; indeed, when left to itself it is almost always on the rise. In pyæmia, on the other hand, it may fall many degrees, so that with the aid of the thermometer alone you will not usually remain long in the dark as to the nature of the case. Again, apart from the thermometer, there is one cardinal criterion which in a multitude of cases will decide the point incontestably. In cerebral rheumatism, at the outburst of a paroxysm, the joint-pains subside or disappear. In pyæmia, if they exist at all, they pursue their own course; they have nothing whatever to do with pyrexia, hyperpyrexia, or head-symptoms. Perhaps I ought to have availed myself of this criterion in W. S., in whom there was no abatement of pain or swelling at the time; perhaps I ought not to have bathed him at all, but I thought it best to be on the safe side, and to give him the benefit of a desperate doubt. It is worthy of note, as contrasting with the results obtained in rheumatic fever, that the bath made no impression upon the nerve-symptoms in our case: the subsultus and the drowsiness were just the same at the close as at the commencement.

I have pronounced the testimony of the thermometer to be an unerring and all-sufficient guide to diagnosis. It is so when the oscillations are present, but it must be borne in mind that they may be exceptionally absent. Their absence, then, offers no decisive presumption against pyæmia, provided there be collateral evidence in its favour. In my own case they were fairly, but not fully, represented. On March 25 there was a long and rapid rise to 105·6°, but there was no corres-

ponding fall before the early morning of the 26th, when the temperature sank to 99° after the bath, which had been administered at midnight. Again, on the evening of the 26th there was a rise to 104·4°. So far, then, the lines on the chart were strongly characteristic, and probably would have been so under any circumstances; but they cannot be taken into the reckoning: the bath intervened and spoilt their true significance. It is well also to remember that the variations may vanish altogether in any case on the approach of death.—*Medical Times and Gazette, Sept.* 6, 1879, *p.* 253.

12.—ON THE TREATMENT OF CANCER OF THE FEMALE GENERATIVE ORGANS BY A NEW METHOD.

By Prof. JOHN CLAY, Obstetric Surgeon to the Queen's Hospital, Birmingham.

As the result of an extended trial of a large number of remedies, both general and local, in the endeavour to cure cancer, the conviction forced itself upon my mind that a remedy for this disease must be one to be administered internally, and that it must be of such a nature as not to interfere with the functions of the special organs, or otherwise to injuriously affect the nutrition of the body, and that it must also be capable of being administered for a length of time sufficient to effect the removal of the disorder. A large number of remedies for the cure of cancer mentioned by various ancient and modern authorities, as well as some hitherto untried, which suggested themselves to me, were employed empirically, but they failed to produce an appreciable effect in curing or arresting the disease. At length, after a careful study of the pathology of cancer, and the effects of certain carbo-hydrates administered internally, it appeared probable that a remedy for cancer might be found amongst them. A list of those apparently most eligible was made, but after studying their therapeutic properties, it became obvious that most of them were unsuitable for the purpose as possessing commonly known specific properties, as well as exciting a special action on certain structures and organs of the body. Besides, their administration could not be expected to be sustained for a sufficient length of time, even if they were likely to act on the morbid growth. Circumstances, however, led me to think differently of the Chian turpentine, and I determined to try it on the first opportunity. This soon occurred. A woman came to the hospital as an out-patient, aged fifty-two. She was suffering from scirrhous cancer of the cervix and body of the uterus. Hemorrhage was excessive, pain of the back and abdomen agonising, and cancerous cachexia well marked. The patient evidently had not a long time to

live. The uterus was so extensively destroyed by the cancerous ulceration that its cavity readily admitted three fingers. In such a case it appeared to be justifiable to attempt to relieve the sufferings of the patient, even if the remedy should produce unfavourable symptoms, or should prove of no avail. I therefore prescribed Chian turpentine, six grains; flowers of sulphur, four grains; to be made into two pills, to be taken every four hours. No opiates were prescribed or lotion used. No change was to be made in her diet or occupation. On the fourth day after taking the medicine the patient reported herself greatly relieved from pain, and was in better spirits, but she complained of a large amount of discharge. It was feared that she referred to a discharge of a sanguineous nature. On examination, however, the vagina was found to be filled with a dirty-white secretion, so tenacious as to be capable of being pulled out rope-like, and this although she had syringed herself three hours previously. The os was quite contracted and would now scarcely admit the finger, and the surrounding swelling or cancerous infiltration of the cervix was much reduced. On the twelfth day the thick tenacious secretion had almost disappeared, and was succeeded by a somewhat copious serous fluid. The os was not so firmly contracted, but would only admit the finger. The patient's general health was improved and the medicine well tolerated. Sixth week: I ordered her a quinine mixture, in conjunction with the turpentine, but sickness supervened, which ceased on omitting the quinine. Twelfth week: My notes are—the parts feel ragged and uneven, and do not bleed on roughly touching them. The speculum shows several cicatricial spots. The turpentine has been taken regularly during the day for twelve weeks every four hours, during which time she has been almost free from pain and has had no hemorrhage; no glandular enlargement; general health improved. Walks easily to the hospital, being about a mile distant. As the patient did not come again to the hospital her address was obtained, and it was ascertained that she had left her residence. Being a widow she could not afford to keep her home, and she went to reside with her married daughter in a northern town; but left no addres. The case showed that the medicine was one of great power in cancer of the uterus, and it is to be regretted that an opportunity was not offered for fully carrying out the treatment.

Another patient, aged thirty-one, suffering from cancer of the os and cervix uteri, was treated concurrently with the one just mentioned. These parts were enlarged from carcinoma to the size of a hen's egg. The os was dilated, and the cavity of the cervix was filled with epithelial growths, which bled freely on examination. Sacral pain was very severe, and hemorrhage

had been continuous for the previous six weeks. The Chian turpentine and sulphur were given as in the previous case. The patient again attended at the hospital on the seventh day after taking the medicine. She was in excellent spirits, and expressed her gratitude for the relief afforded her. The medicine entirely relieved her pain. She had increased white discharge. On examination the os and cervix were found to be nearly of the normal size. The os was patulous, and its surface was studded with flabby shot-like eminences, which did not bleed on roughly rubbing them. I said to her, "You are better; you must continue the medicine." She answered, "I should think I must, for I could not do without the pills; they have eased me so very much." She continued to improve, and on the fourth week she expressed herself as quite well. I impressed upon her the necessity of continuing the medicine and told her to see me occasionally. She did not come to the hospital again for four months, when she brought another patient to consult me, believing that she was suffering from cancer. I reproved her for leaving off attendance at the hospital. She answered that she thought it unnecessary, as she had continued quite well. On this visit she submitted to an examination. The os was rough and irregular, but was of nearly the normal size; no signs of cancerous infiltration; the periods were regular, and not profuse, and were unattended with pain; there was slight leucorrhœa. This case was a most remarkable one. The turpentine acted upon the growth with great vigour, literally melting it away in the brief period of four or five weeks.

The third case was one of epithelial cancer of the os, cervix, and body of the uterus, in a woman, aged fifty-two years. The vagina was not involved. The mass was larger than a cricket-ball, almost filling the vagina. The border of the os was three-quarters of an inch in thickness, forming a ring of two and a half inches in diameter, through which protruded an epithelial growth, principally proceeding from the anterior wall of the uterus, and projecting about two and a half inches into the vagina. The case was sent to the hospital for my opinion by my son, Mr. Langsford Clay, who had attended the patient but a short time. The journey to the hospital fatigued her very much, and she declared that she could not come again, and that she did not wish to remain as an in-patient, believing that she could not live many days. She had repeated hemorrhages, had much pain, and had the cancerous cachexia well pronounced. My son volunteered to attend her at home, and I agreed to see her occasionally with him. I thought it advisable as an experiment to vary the treatment somewhat, and ordered to be added to the pills one-sixth of a grain of the ammoniated

copper, as from the large mass to be acted upon I thought that an astringent should be superadded to the turpentine. The dirty-white tenacious discharge appeared and continued for the first five weeks, but there was no hemorrhage after the first examination. The swollen os uteri and the cervix beyond were the first to show signs of diminution; this was noted on the fourteenth day. The tumour, however, was rough and shrunken, and did not project so much. Sixth week: The surface of the tumour was at the level of the os uteri, and seemed to consist of a mass of bloodvessels which bled moderately after examination. This condition occasioned me some surprise, as three weeks previously the patient was ordered a lotion made with perchloride of iron, with a view to arrest hemorrhage, since from her anæmic condition it was feared that the loss of a moderate amount of blood would be followed by serious consequences. I asked her what kind of syringe she used with the lotion. She replied, "I thought the lotion was merely to bathe the external parts." This, as it happened, was very satisfactory information, as it showed that the lotion had no share in the reduction of the mass, which now was scarcely half the original size. She was supplied with a syringe for the purpose of applying the lotion, and after using it three days the mass of vessels had considerably shrunken, and no longer bled on manipulation; but the surface of the growth had the touch and appearance of a gangrenous mass, but there was scarcely any fetor. The patient now complained of gastrodynia, with colicky pains in the bowels, but she had no diarrhœa or vomiting. I believed this to be due to the copper, and it was consequently discontinued. It also occurred to me that the turpentine might not be efficiently digested in the solid form, and that it would be better if the remedy were administered in a state of minute subdivision, as in the form of an emulsion. An ethereal solution of Chian turpentine was prepared by dissolving one ounce of the turpentine in two ounces of pure sulphuric ether (anæsthetic). The ether dissolved the turpentine instantly. This solution was given to our skilful dispenser, Mr. Whinfield, with a request that he would prepare a pleasant mixture or emulsion from it, and after a few trials he prepared one which is not unpleasant to take, according to the following formula:—Solution of Chian turpentine, half an ounce; solution of tragacanth, four ounces; syrup, one ounce; flowers of sulphur, forty grains; water to sixteen ounces: one ounce three times daily. This form of mixture was given to the patient, and was much liked. She has now taken the turpentine for thirteen weeks uninterruptedly. The os uteri is a little more than one inch in diameter, and feels like a ring of cartilage about a quarter of an inch in thickness. The tumour

has nearly disappeared, and the finger can be introduced posteriorly into the uterus for more than an inch. The general health has much improved, and she is quite free from pain and looks cheerful, and is becoming stouter. No sedative whatever has been given during the treatment. Fourteenth week: She complained of severe "cramp-like pains" in the back and lower part of the abdomen, which she attributed to the mixture, and in consequence it was discontinued for a few days, and an opiate given, by which she was greatly relieved. The turpentine was again resumed. Nineteenth week: She is now fairly convalescent. The growth has almost disappeared, and the parts beyond the os uteri are somewhat hypertrophied, yet are almost normal to the touch.

The fourth case was that of a patient aged thirty-two years, who came to the hospital after having been discharged as incurable from the Women's Hospital. She was greatly depressed, and was most desirous to be cured, for the sake of her family of young children. She has had repeated floodings, and suffered greatly from pain during the past five months. Constipation very troublesome, which probably arose from the opiates she had been in the habit of taking. On examination, she was found to be suffering from epithelial cancer of the os and cervix uteri, but not involving the vagina. There was a cancerous mass of the posterior parts of the os and cervix, of the size of a goose-egg. This growth pushed the os uteri towards the pubis, almost preventing that part from being felt. The turpentine mixture was given three times daily, and from this period a very rapid diminution of the growth took place, so that by the sixteenth day it had almost entirely disappeared. The os uteri was now *in situ*, admitting the finger readily, and there was the same condition of the vessels as that observed in the preceding case. The lotion with the perchloride of iron was used daily for a few days with excellent effect. In the ninth week the patient suffered from spasmodic pains in the back and abdomen, and as this was attributed to the medicine it was discontinued, and iodide of calcium, in five-grain doses, three times daily, was administered. This was taken for about a fortnight, but, not feeling so well, the patient was admitted into the hospital. The condition of the internal organs was now much the same as before, the iodide of calcium was given, but there was some thickening about the cervix, which was fixed to the vagina. The rectum was excessively loaded, and required several days to effectually relieve it. The Chian turpentine was administered simply; but a lotion was prescribed, containing six grains of white arsenic to one pint of water, to be used daily. Under this treatment the woman very rapidly improved, the pains entirely ceased, and the parts became much

reduced in size and more movable. The patient was now anxious to leave the hospital for her home, as she felt quite well; but it was deemed advisable to send her to the Sanatorium instead. She is very active, cheerful, and happy, and may be pronounced convalescent.

Other cases are under treatment, both in the hospital and privately, all showing similar effects. The remedy is now being tried in cancer of other organs, and apparently with good results. One of the most interesting, perhaps, is a case of scirrhus of the breast, which has been under observation for some weeks. Among the other cases are cancer of the vulva, stomach, and abdomen, in which very remarkable benefit has been already produced.

From the results obtained by the use of Chian turpentine, it may be confidently said that the remedy does exert a powerful action on cancer of the female generative organs in particular, and it will be of advantage to point out some of the conclusions at which I have arrived respecting the efficacy of the drug and the manner in which it should be employed. The oil of turpentine, if it produces any effect on cancer, is inadmissible on account of the speedy production of its specific effects even when administered in small doses. The same remark applies with less force to the Venice and Strasbourg turpentines; in my hands they have not produced the same beneficial effects on cancerous growths as the Chian turpentine has done. The maximum dose of the last-named drug which can be safely and continuously given is twenty-five grains daily. It is advisable to discontinue the remedy for a few days after ten or twelve weeks' constant administration, and then to resume it as before. The combination with sulphur was given at first, and has been continued. It is doubtful whether much benefit is derived from the combination, but the effects have been so uniformly good with it, that it was thought advisable to continue its use. There is every reason to believe, from the trials made with other substances in combination with the turpentine, such as carbonate of lime, iodide of calcium, ammoniated copper, quinine, bebeerine, hydrastin, &c., that the turpentine is best administered simply, as the most marked and rapid effects have always been manifested when it has been given alone.

The turpentine appears to act upon the periphery of the growth with great vigour, causing the speedy disappearance of what is usually termed the cancerous infiltration, and thereby arresting the further development of the tumour. It produces equally efficient results on the whole mass, seemingly destroying its vitality, but more slowly. It appears to dissolve all the cancer cells, leaving the vessels to become subsequently atrophied, and the firmer structures to gradually gain a comparatively normal condition.

It is a most efficient anodyne, causing an entire cessation of pain in a few days, and far more effectually than any sedative that I have ever given. In the cases I have described no sedative was employed in any instance, although in some cases where great pain had existed previously to commencing the treatment, large doses had been given. Whether this arrest of pain arises from the death of the tumour, or, as my son suggests, is due to there being no longer irritation of the sentient nerves (in consequence of tension being withdrawn by the removal of the cells), the fact is the same.

If, after the use of the remedy for some weeks, one of these cases were examined by a stranger for the first time, he would probably conclude that it was one of commencing malignant disease, by reason of the irregularities of its surface. The effect of the remedy being first to remove the cellular structures, any loss of tissue produced by the invasion of the disease cannot be restored, and hence the irregular touch and appearance even after cicatrisation. The arrest of the hemorrhagic discharge and the remarkable freedom from glandular affections, after a lengthened use of the turpentine, are specially important factors in materially aiding the removal of the cachexia and of improving the general condition of the patient.

Without being in a position to affirm that the Chian turpentine is a positive cure for advanced cancer of the female generative organs, yet however the facts here adduced may be interpreted in this respect, two circumstances are indisputable—one, that all the patients after several months' treatment are living, and that the disease has not advanced as is usually the case, but has retrogressed—in fact, has all but disappeared; and it may at least be safely asserted that when the remedy is steadily used for some time it arrests the progress of the disease, and relieves the pain incidental to the morbid growth in a manner which cannot be said of any other remedy. It is probable that on an extended experience of its use and by variations of the mode of administration, it may prove an effectual cure for this intractable disorder. Patience and perseverance on the part of patient and medical adviser are absolutely required. We know that in some diseases, as bronchocele and syphilis, a long continuance of well-known remedies is often necessary to effect a cure of the particular disorder, and that the administration of the remedies has to be varied from time to time, according to the therapeutic effects produced by the drugs. In cancer, as far as experience has at present indicated, the same alternating method may perhaps have to be employed. Whatever may be the ultimate results there can be no doubt that Chian turpentine in these disorders is a most valuable medicine. Judging by my experience it is no figurative expres-

on to say that it acts as a direct poison upon the growth is probably causing its ultimate death. In advanced cancer the process of reparation is slow, but if the surrounding structures are not too much involved in the process of destruction, it will seem that a cure may be reasonably expected. It is not that the remedy has failed against the cancer, but that the vital organs are so much destroyed that their complete reconstruction and adjustment of functions are not possible, and life fails in consequence of their mutilated condition. Even under these circumstances, if the cancer does not recur, the efficacy of the medicine is obvious. In the early stages of cancer it may be affirmed that an undoubted cure may take place speedily, and as the contiguous structures are not extensively involved, but little deformity ensues; and experience justifies the expectation that under such circumstances a recurrence of the disease will not follow.

The history of the local treatment of cancer of the uterus is one of singular interest, and is highly instructive to the practical physician. The contrast between the general and local treatment is the more notable, as nothing can be more injurious to the welfare of the patient than an attempt to destroy the cancer by external agencies. The disease is not to be averted by this means, as the symptoms assume a more intense and threatening character, until the patient rapidly sinks. It may be observed that the internal treatment here recommended when used for a considerable period is borne by the patient with remarkable tolerance. As I have mentioned, in some of my experiments I determined, in order most thoroughly to test the medicine, to rely upon this alone. Recently the arsenical lotion has been superadded, and with no injurious consequences—it appears to act as a disinfectant, and it may produce some benefit by promoting the cicatrisation of the tissues. Several suggestions offer themselves for inquiry as to aiding locally the detachment of the growth, after its vitality has been destroyed; but this is not of much importance, as there seems to be no fear of the blood becoming affected by the absorption of the decaying tissues, the turpentine probably preventing any such calamitous occurrence.

If the practice now described should prove by future experience to be justified, then it will be incumbent upon the medical adviser to treat cancer of the generative organs at an early stage of its development, and it is reasonable to conclude that this dreaded and most fatal disease will no longer be the scourge it has hitherto proved, and that another benefit will have been conferred upon suffering humanity by the resources of therapeutic art.—*Lancet, March* 27, 1880, *p.* 477.

13.—ON THE NEW TREATMENT FOR CANCER.

By the EDITOR OF THE LANCET.

The remarkable statement made by Professor Clay, of Birmingham, in the preceding article on the "Treatment of Cancer by a New Method," has naturally excited great attention, and will doubtless induce hospital physicians and surgeons, and practitioners generally, to make a prompt trial of the remedy, which is declared to have achieved highly satisfactory results in the relief, if not also the *cure*, of a disease that has hitherto baffled treatment. The matter is evidently one of more than common moment to the profession and the public. It will be important to make sure that the drug employed is certainly what it professes to be. As a matter of fact, it is by no means easy to obtain Chian, or Cyprus turpentine. So long ago as the date of the publication of the earlier editions of Pereira's Materia Medica there was so much difficulty in procuring the substance that the profession was especially warned against the almost inevitable substitution of Venice or Canada turpentine, or some other terebinth of totally different properties, in dispensing of prescriptions, for Chian turpentine. Probably there is scarcely any of the true resin in the market at present, and only druggists who happen to possess a small forgotten store *can* supply it. We think it desirable to make this intimation for the sake of medical practitioners who may be anxious to try the remedy, but who are almost sure to be disappointed, unless they take more than ordinary measures to ensure accuracy. As Professor Clay stated in his paper, no other terebinth except the Chian has been known, or can be expected, to produce the effects which have followed its use in his cases. The public should also be cautioned against resorting to the remedy, without skilled medical advice and supervision; and very earnestly must they be made to understand that probably not a tithe of the medicine likely to be sold under the name of "Chian turpentine" during the next few months will be genuine. The caution given to the profession years ago by the then highest authority on the subject of pharmacy, at a time when there was no special demand for the drug, will be doubly necessary now, when nothing but the accident of having a small reserve on hand can enable any druggist to dispense it. It only remains to add that some of the received descriptions of *Terebinthina Chia* seu *Cypria* are so faulty that identification of the genuine drug will be attended with more than ordinary difficulty. It is the gum of a tree growing thirty or thirty-five feet in height, and is obtained by cutting crosswise with a hatchet the trunks of the largest trees. The yield is very small, "not exceeding eight or ten ounces" for

each tree; so that it must be obvious that the bulk of the material sold under this designation at two or three shillings a pound is not genuine. Its consistency is that of honey, but it is more glutinous. The colour is greenish-yellow. It has an agreeable turpentine-like odour, combined with the odour of fennel or citron and jasmine. Its taste is very mild. By keeping it resinifies, and, we may add, loses some of its virtue. The coniferous turpentines, which do not possess its special qualities, are usually sold for it.—*Lancet, April* 3, 1880, *p.* 534.

14.—ON THE TREATMENT OF ACUTE RHEUMATISM BY SALICYLATE OF SODA.

By Dr. REGINALD SOUTHEY, Physician to St. Bartholomew's Hospital.

The outcome of our observations leads us to the inference that two varieties of acute rheumatism exist, and may be distinguished as continued and as relapsing forms of the disease.

The continued form, as shown by Sir William W. Gull and Dr. Sutton, is apparently wholly uninfluenced in its course or complications by the alkaline treatment, or any other so-called special cure hitherto employed, whereas the relapsing form could hardly be adjudged, upon the evidence hitherto produced, to be either shortened in its course or deprived of its cardiac perils by either alkalies, iron, quinine, or blisters.

The treatment of the disease by rest, by a comfortable bed, by good hygienic measures, by careful nursing, by local and general anodynes, was apparently all that can be done, and notwithstanding what has been advanced to the contrary, so far as our hospital statistics dealt with the matter, no specific cure for rheumatism appeared to exist, while all drugs hitherto administered—salicylic acid and its congeners not excepted—seemed palliative rather than curative in their action.

We promised, however, to examine the treatment of the disease by salicylate of soda statistically, and redeem our word by the table which accompanies this communication.

It becomes necessary to call attention to the two forms of rheumatism already mentioned, since erroneous conclusions as to the effects of special remedies appear to us fostered by ignorance of the fact that one class of these cases tends to recover early and quite spontaneously, while the other, by reason of a sudden abatement of pains and fever, gives the semblance of yielding to remedies which really exercise little or no influence over it, the cases relapsing and remaining in hospital every bit as long when treated by salicylate as by other specifics up to this period essayed.

The following features characterised the continued form.

Uncomplicated by either pericarditis or pleuro-pneumonia, its ordinary accidents, a subsidence of pains with remission of temperature takes place gradually on the eighth and ninth days, and by about the twelfth day of the disease, counting this from the first day in bed, the patient feels well but weak. Convalescence is rapid, and between the fourteenth and twenty-first day the case is ordinarily discharged well.

The relapsing forms, fully as suffering when first seen, have generally been ill longer; they may have taken to their beds quite as early in the complaint, but have usually made an effort to get up and be about again. These cases, when they first fall under observation, seldom fail to present an endocardial murmur.

It is our opinion that endocarditis is as much a feature of the disease as the articular inflammations; between the ages of eight and twenty-five scarcely ever absent, although often overlooked. No murmur may betray its presence until the swelling of the endocardium subsides or deposition of fibrin takes place upon the roughened serous membrane, but the heart sounds are never like those of perfect health, clear and sharp, and when convalescence is reached, a distinct murmur is manifested. In this form, between the fifth and eighth days the pains remit with singular invariableness, except the patient has been suddenly purged or otherwise disturbed, and by the evening of the eighth day the temperature is nearly or quite normal. On the evening of the twelfth day the temperature rises slightly to 99.5° or 100.2°, and on the morning of the thirteenth there is a slight relapse with articular pains, lasting from twenty-four to forty-eight hours. But again the pains and fever subside, and the case either enters upon a tardy convalescence about the twenty-first day, or relapses again and again after various delusive periods of remission, so that the mean average residence of a relapsing case in the hospital is forty-two days.

Salicylate of soda is far from being unobjectionable as a remedy. In four cases I was obliged to stop its administration on account of sickness, and just as many times by reason of salicylism, by which term I connote the usual aggregate of disagreeable symptoms, faintness, deafness, and small running pulse, to which the attention of the profession has been already drawn. Twice when it was given violent maniacal delirium appeared due to its administration, for the delirium subsided after withdrawal of the remedy.

We are far from denying potency to salicylate of soda administered in the frequent and large doses which have been

recommended; we are of opinion that the pulse rate is increased and the blood tension diminished by its action; at all events, the pulse becomes less full and throbbing, and the drug exerts a general soothing or anodyne influence. The duration of an attack of acute rheumatism does not, however, from our statistical examination of the remedy, appear at all shortened by it; nor is the tendency to heart complication diminished, nor is the endocarditis apparently bereft by it of its ultimate damaging effects.

Those who fancy salicylate of soda is a specific cure for rheumatism have in our opinion failed to realise the fact that the acute continued forms of this malady are *ipsâ naturâ* short-lasting cases, and that the relapsing forms under ordinary rational treatment exhibit sudden intermissions of fever and local inflammatory action lasting for days and even weeks, like indefinite forms of ague with relapsing tendencies, wholly apart from any drug treatment whatsoever.

Against the adduction of some of the cases recorded in my table as evidence objection may be raised, forasmuch as the disease had lasted some days before the salicylate treatment was commenced.

It is inevitable, however, that this should be so. We can only state our experience as this has been gathered. Relatively few cases are admitted into hospital earlier than the fifth day. But this circumstance tells both ways, as much for as against the remedy, since the natural tendency of the acute cases proves that a remedy administered between the sixth and ninth days is likely to be credited with more merit than strictly belongs to it.

In the relapsing form, twenty-four hours' rest in bed after a bath, and any treatment except free purgation, between the sixth and eighth days of the disease is attended by a marvellous subsidence of pains. This, we think, has been mistaken for an effect of treatment upon the disease.

If salicylate of soda were such a specific for acute rheumatism as has been asserted, we are of opinion that it should exhibit its power of controlling or curtailing the disease in more manifest terms. Neither of these expectations, however, have been fulfilled.

There are few diseases, perhaps, which present more dangerous pitfalls for the unwary and clinically ill-informed therapeutist than rheumatism does, and it is only just to point this out from the statistical survey of cases specially treated.—*St. Bartholomew's Hospital Reports, vol.* xv., 1879, *p.* 7.

15.—ON THE ALKALINE, SALICIN, AND SALICYLATE OF SODA TREATMENT OF ACUTE RHEUMATISM.

By Dr. ROBERT SINCLAIR, Physician to the Dundee Royal Infirmary, and Physician to the Aural Department.

In the Lancet of Jan. 8th, 1876, there appeared a short paragraph containing a sketch of the results obtained by Dr. Reiss, of the Berlin Metropolitan Hospital, in the treatment of 400 cases of febrile disease by means of the salicylate of soda. Briefly stated, the effects were—rapid reduction of temperature; hastened convalescence; frequently profuse perspiration; occasionally tinnitus aurium, dimness of vision and vomiting; no effect on the pulse; antipyretic effects in inverse ratio to the fatality of the diseases.

About the same time Professor Traube and Staff-Surgeon Stricker were making trials of the drug in febrile diseases, and especially in acute rheumatism. The results exceeded their expectations. Dr. Stricker very early came to the same conclusion in regard to salicylic acid, that Dr. Maclagan formed regarding salicin—namely, that the more acute the attack the better the result. His plan has usually been to administer the drug in fifteen to thirty-grain doses, every hour for six hours. By the end of this time the patient is usually ready to fall asleep, and may be left undisturbed for some time. The same, or a similar quantity, is given daily till recovery is ensured. "All the patients thus treated," says Dr. Stricker, "were not only relieved of their fever, but also of their local symptoms—*i.e.*, the swelling, redness, and especially the painfulness of their joints within forty-eight hours, most of them even within a much shorter period." This statement is based on the observation of fourteen cases. Dr. Hermann Senator, of Berlin, and others in Germany have come to similar conclusions, which for the most part have been stated with great moderation. The acid and the salts appear to be used indifferently by many physicians; but the salicylate of soda is now the favourite form, in this country at least. The tendency to cardiac complications is believed to be diminished only in proportion to the reduction of fever; and their presence is little if at all influenced by the drug. Chemically pure preparations must, it need hardly be said, only be used. The toxical effects observed in several cases—namely, deafness, giddiness, vomiting, sighing respiration, delirium, and depression of the cardiac action, as well as the hemorrhagic erosions of the mucous membrane of the stomach and intestines, discovered by Drs. Wolffberg and Reiss, are probably due in a measure to adulteration with carbolic acid; but they have not been wanting in some cases treated with perfectly pure preparations.

The eighth volume of the St. George's Hospital Reports (1874-76) contains two excellent papers on the treatment of rheumatism by salicin and salicylate of soda. One is by Drs. Whipham and Collyns, the other by Dr. Cavafy.

The cases of Drs. Whipham and Collyns are of little or no value for statistical purposes, for the following reasons:—The number of cases is too limited; some of them were evidently acute, some subacute, some chronic; five only were subjected to salicylate of potash alone, one to salicin alone, two to both drugs at different times, one to salicin, iodide of potassium, and iron, and one to salicin, iodide of potassium, iron, and a blister. But the cases are nevertheless instructive in other ways. In one (case 8) pericarditis was developed three days after the commencement of treatment by salicylate of potash. In another (case 9) some cardiac affection, the nature and termination of which are not at all clear from the report, was developed under treatment by salicin. One thing is clear enough, however, that salicin was not given in sufficient doses. Of seventeen cases of rheumatism and other diseases treated by salicylate of soda or potash, nine suffered from toxic symptoms.

Dr. Cavafy's paper is quite another kind. He records twenty-one cases treated by one drug—namely, salicylate of soda, and the salient points of each case are stated with great distinctness. Moreover, his results are, in the main, a fair illustration of what the drug can do, and of what it has done, in the experience of the profession during the last three years and a half.

The first thing that strikes one on examining Dr. Cavafy's careful reports and charts is the apparently rapid effect of the drug in relieving pain and reducing temperature; and if this were all that is wanted, we might well congratulate ourselves that "we at last possess a most efficacious remedy for acute rheumatism. "But here I, for one, am prepared to join issue with Dr. Cavafy. We want something more. We want a remedy that will speedily free our patients from the stiffness, the anæmia, and the weakness incidental to the disease, and a remedy, moreover, that will enable them to resume their ordinary occupations and pleasures at the earliest possible moment. Let us see how far the salicylate of soda fulfils these objects. The average duration of the rheumatic symptoms—namely, pain, swelling, and elevation of temperature—was 11·4 days; and the average residence of the patients in hospital was 25·3 days. Dr. Fuller, on the other hand, found the average residence of his patients in hospital, under the alkaline treatment, to be 20·1 days; and if we include twenty-eight patients who had pericarditis on admission, it was still only 22·2 days. I am

quite aware that Dr. Cavafy may object to the total duration both of the attacks and intermissions of his relapsing cases being included under the column headed "Duration under Treatment." But my reply to this objection is, that I have taken all his cases into calculation, some of them evidently exceedingly trifling attacks; and I repeat that the drug must be tested by its power of *preventing* relapses and producing speedy convalescence. It has been already shown by Dr. Southey, to whose paper reference will shortly be made, that the *attacks* in relapsing rheumatism tend to terminate without treatment. The prevention of the *occurrence of relapses* has not been assisted in the slightest degree by the salicylate of soda, either in Dr. Cavafy's cases or mine. It would no doubt be much more satisfactory to separate the continued, from the relapsing cases in our calculations and comparisons; and I hope this will be done in all future publications. But whenever we compare our results nowadays with those of the less recent systems, we must, in the interests of truth and fairness, take continued and relapsing cases together, for the simple reason that in former statistical returns the separation has not been made. There are only two other points in these twenty-one cases to which I would draw attention. Although, in a very small minority, cardiac murmurs were developed under treatment, it does not appear that any grave cardiac complication occurred in a single case. On this result Dr. Cavafy is to be congratulated. The other point is, that toxic symptoms, some of them very serious, occurred in thirteen of his twenty-one cases. Without pretending to explain the cause of the protracted convalescence which many of us have observed in certain cases of rheumatism treated by the salicylate of soda, I should like to throw out the hint that the depressing effects of this drug may have had some share in the unhappy result.

One of the most valuable contributions ever made to our knowledge of the natural history of acute rheumatism is to be found in the St. Bartholomew's Hospital Reports, from the pen of Dr. Southey, who advises his brethren in attempting to form an estimate of the effects of any remedy, new or old, to distinguish carefully between two forms of the disease, the continued and the relapsing. The former, he believes, usually remits about the eighth or ninth day, and about the twenty-first day the patient is able to be out of bed. The latter remits about the seventh day, whatever treatment be adopted. But, while in the former variety convalescence usually proceeds satisfactorily, in the latter the patient, after being apparently well, and perhaps out of bed, is again seized with rheumatic inflammation of his joints, which once more remits quite suddenly on or about the fourteenth day from the first seizure. This

remission may be permanent, and usually is a longer remission than the first, but it may be succeeded by a second, a third, or even a fourth relapse. Dr. Southey does not appear to be so enthusiastic about the latest additions to the *materia medica* of rheumatism as some of our brethren, for he states that though salicylate of soda has done some good, it has never, in his experience, prevented relapses. This periodicity he recommends us to attack with large doses of quinine.

Dr. Julius Pollock, of the Charing-cross Hospital, assures us that he has used salicin without much success; but, he adds, "the evidence in favour of salicylate of soda in the treatment of articular rheumatism is becoming so overwhelming that its great value must shortly be thoroughly established." Dr. Pollock is convinced of the efficacy of the drug, it should be said, in *all* forms of articular rheumatism. He usually gives it in combination with "two or three grains of carbonate of ammonia in an ounce of camphor water, or with tincture or syrup of orange, in orange flower water," and he always gives it freely. In the appendix to the monograph quoted above, Dr. Pollock records his notes of sixteen cases, fourteen being acute, and two subacute rheumatism. The results show, for the most part, that salicylate of soda has considerable influence in reducing fever, abolishing pain, and in many cases bringing about a speedy convalescence. But they show something else that should be carefully remembered by the salicylic zealots. They show three fatal cases in a total of sixteen, and they prove incontestably that the drug does not invariably prevent pericarditis, endocarditis, pleurisy, and hyperpyrexia.

In a survey of the chaotic literature of salicylic acid, we cannot omit reference to one of the earliest papers on the subject, by so well-known a writer as Dr. Broadbent, of St. Mary's Hospital. Whatever may have been the extent of Dr. Broadbent's experience at the time he penned the article of April 8th, 1876, his published cases amounted to four, the rapid and satisfactory terminations of which, under salicylic acid, are undeniable. Such results were no doubt a sufficient excuse for a little comfortable comment. But they by no means palliate the injudicious haste with which Dr. Broadbent rushed to the conclusion that, "according to present experience, rheumatic fever, when treated by this drug is an affair of two or three days"; a remark that was shortly afterwards echoed by a similar one in regard to salicin, by Dr. Maclagan—"Seeing a patient suffering from acute rheumatism I have no hesitation in assuring him that within forty-eight hours, possibly in twenty-four, he will be free from pain."

Dogma like this cannot be sufficiently deplored. It hinders the progress of therapeutics, imperils human life, and in the

long run injures the cause it is intended to help. Unfortunately, the student of the history of medicine is no stranger to confident assertions; some with good foundation, some with marvellously little, and some with none. Dr. Broadbent is surely aware of the good results of the alkaline treatment in the hands of Fuller, Garrod, Furnivall, and Dickinson, and he cannot forget that Dr. Hope, after six years' experience, in 200 cases, preferred a line of treatment that included moderate bleeding, calomel, opium, colchicum, and salines, for the excellent reason that, thus treated, his patients were usually well within a week, and able to resume their occupations in ten days after the cessation of pain. These facts are not mentioned with the view of depreciating the undoubted value, in certain cases, of salicin and the salts of salicylic acid, but merely for the sake of keeping in view the important truth, that the rapid cure of acute rheumatism is not altogether new.—*Lancet, Feb.* 14, 1880, *p.* 244.

16.—ON THE ACTION OF SALICIN AND SALICYLIC ACID IN ACUTE RHEUMATISM.

By Dr. WILLIAM SQUIRE, F.R.C.P.

The treatment of rheumatic fever with iron has certain advantages over that by alkalies, and many methods show better results than the expectant; none of them, before the use of salicylic acid, would markedly lower the fever and shorten its duration, or lessen the liability to heart disease, or diminish the anæmia of convalescence, with its tendency to relapse, nor will this means altogether prevent these risks. In all cases of acute rheumatism we find—(1) increased tissue-change; (2) checked elimination; (3) ready febrile reaction of a special character. To these three conditions treatment has always been directed, the success of the means employed being very much in proportion to the power exercised over the last. As the fever is intensified by the local accident to which it gives rise. direct applications to the joints are useful. Whatever relieves pain reduces fever, and local vascular tension has here to be relieved; only when this is very limited is colchicum or aconite of use. Bleeding and purgatives fail to do good, as all evacuants fail when carried beyond what tends to restore the balance of elimination. Any excess of fibrin in the blood is not to be prevented by such means. Hyperinosis is a direct consequence of the febrile state, and is diminished by whatever controls fever; fever is actually increased by some eliminative means, as by sweating under blankets; quinine, and even digitalis, have been used with advantage in the fever. Against the hyperpyrexia, sometimes met with, the cold bath is effective, and to this condition its use is restricted. In all plans of treatment complete rest is

essential; liquid nourishment or milk diet is required, with free use of diluents.

Details of eight cases treated by iron, and of eight treated by the salicylates, were brought forward. Of the former series, one case, following mumps, began with pericarditis, and one had endocarditis after the puerperal state; of the latter series, one case, following measles, had no cardiac affection, and the others, after varicella, began with endocarditis. This is often the first sign of rheumatism in children; in their slighter febrile ailments the heart should always be examined. Rheumatism is readily induced where tissue-change has been rapid, It may occur so early in scarlet fever as to be considered part of the disease. After diphtheria, and whenever, in children, acute rheumatism has followed quickly on the specific fever, the iron treatment is specially useful. Nor need cardiac mischief preclude its use: it acts more as a sedative than as a tonic, and diminishes irritability, while it favourably modifies the inflammatory products; any increase of blood-pressure can be kept down by laxatives and perfect rest in bed, or further lowered by chloral at night. The great use of salicylic acid is, that by shortening the fever the risk of heart complication is lessened; and if the heart be already affected, no remedy can act better. It slows the pulse, lowers the blood-pressure, and diminishes vascular tension; the fever is controlled better than by quinine; pain is relieved better than by sedatives; no secretion is checked: the natural crisis of the disease is hastened; the subsequent anæmia is less, and the convalescence quicker, than after treatment by iron. Kidney disease, chronic or acute, is an obstacle to the free employment of salicylic acid. Other conditions may arise where a choice has to be made between the use of iron and of the salicylates; one or the other must be adopted, they cannot both be used at once.

Salicylate of soda may be given to patients at all ages, and in all stages of the disease; not only is cardiac disturbance quieted, but the pulmonary congestion, meteorismus, diarrhœa, and profuse perspiration of persistent rheumatic fever in debilitated persons, are relieved, and the pale urine, deficient in urea, in these prolonged cases, is soon restored to its normal quality. In acute rheumatism salicylate of soda is preferred to salicylic acid; it is readily soluble, is neither irritant nor disagreeable if well diluted, is more readily absorbed, and its effects are much more prompt, certain, and manageable. Five grains of the salt equal four grains of the acid, a sufficient dose for a child of six or eight years old; adults require three or four times this quantity. It must be given every two or three hours till we have some evidence of its action; this is to be looked for after three or four doses. When six or eight have been

given in this way they need only be continued every four or six hours for another day, and can be resumed in the same way if fresh pain or fever arise. Salicylic acid must be converted into salicylate of soda in the blood before its action on rheumatic fever begins; a definite quantity of the acid can be dissolved in presence of potash, lithia, or ammonia, and be so given with or without effervescence.

Among the secondary or indirect effects of the salicylates useful in rheumatism is that of increasing the excretion of urea and favouring the elimination of uric acid. M. Germain Sée noticed this in chronic rheumatism; and more recently M. Marrot has shown that in the acute attack this increased excretion appears before the joint swelling subsides, and continues after the fever has fallen; so that not merely a removal of febrile waste but a true crisis is induced, similar to what may always be observed before the termination of rheumatic fever. Another good effect is the relief of pain, either by soothing the peripheral nerves or relaxing the small vessels by acting on the vaso-motor centres. The lowered temperature is from its effect on the nerve centres; so is the tinnitus aurium. It slows the pulse and respiration, for it is a pneumogastric sedative, and tones the heart, while it lessens vascular resistance. Contrary to what is seen in other fevers, instead of the dilated vessels allowing a greater production and a greater discharge of heat, the local liberation of salicylic acid checks molecular change, and the production of heat is diminished. Further evidence of this was given in proof of the greater rapidity with which salicylic acid is disengaged and eliminated during an attack than after it is over; hence, during fever a larger quantity of the drug may be well borne, but its action is also more rapid, and requires to be carefully watched.

Many objections to the use of this remedy have disappeared since the dose has been more accurately determined. Less than half a drachm of the acid, or forty grains of salicylate of soda, taken daily by a healthy man, will not produce any marked effects, but fifty grains of the acid or a drachm of its soda salt, taken continuously for two days, will do so; the effects of one drachm of the acid taken at a single dose will also be felt for two days. Sixty grains given to a boy six years old in typhoid fever caused great depression for two days. A drachm of salicylate of soda given one afternoon to a young woman with rheumatic fever caused delirium, which subsided in three hours. A characteristic of nervous symptoms produced by the salicylates is that they subside quickly on discontinuing the medicine, even when during some days such quantities as 360 or 600 grains have been taken, and have produced delirium and albuminuria. Extreme effects produced by 340

grains, given in six hours, by mistake, to a girl at Kiel, were recovered from in ten days. Some large doses of salicin and salicylic acid, said to be harmless, were probably not all absorbed. Fifteen grains of salicylic acid is the smallest dose reported as producing nervous symptoms. Delirium occurred in two of my cases, and some deafness in one, before giving the salicylates. Symptoms of disease, such as of cerebral rheumatism and of embolism, have been attributed unjustly to salicylic acid. The continuance of extreme doses is of no service in rheumatism, and may be dangerous in typhoid or in erysipelas from pulmonary congestion being either masked or increased.—*Lancet*, *Dec.* 20, 1879, *p.* 905.

DISEASES OF THE NERVOUS SYSTEM.

17.—ELECTRICITY AS A METHOD OF PHYSICAL DIAGNOSIS IN DISEASES OF THE NERVOUS SYSTEM.

By Dr. A. HUGHES BENNETT, M.R.C.P., Physician to the Hospital for Epilepsy and Paralysis, Regent's Park; Assistant-Physician to the Westminster Hospital, &c.

For practical purposes, two forms of electricity are employed; namely, the induced or faradic, and the continuous or galvanic currents. The sources from which these are derived, as well as their physical and chemical properties, entirely differ from one another; but with these and other general considerations it must at present be assumed that you are acquainted, and we must devote our attention to their physiological and pathological actions.

In order that electrical reactions in disease may be thoroughly appreciated, a preliminary knowledge of their behaviour in health is essential. We shall therefore discuss the subject under the following heads: 1. The actions of the faradic and galvanic currents upon healthy living tissues; 2. Their actions on injured or diseased living tissues; and 3. The utilisation of our knowledge on these questions for the purposes of diagnosis.

1. *The Actions of Faradism and Galvanism upon Healthy Living Tissues.*—In all the tissues of a living animal where active nutrition is being carried on, there are natural currents of electricity constantly flowing, the character and direction of which can be determined by a delicate galvanometer. In nerve and muscle, this electro-motive force is the greatest; and in them, in common with the other tissues, their interior and exterior aspects are inversely electrified, the current flowing

from the latter to the former, from longitudinal to transverse surfaces, and, in the human subject, from the feet to the head. This is a matter of some importance; for, when a current of electricity from a battery is sent in the same direction as the natural animal current, the functional irritability of the tissues is augmented, but if in an opposite direction it is diminished.

The physiological effects of electricity depend upon the form, intensity, mode, and length of application, and to the organ and vitality of tissue which come under its influence. The *faradic* current is a powerful nerve-stimulant, and, like all other stimulants if used in excess, it paralyses in large, what it stimulates in small, doses. Consisting, as it does, of a series of rapidly repeated shocks, it has little or no chemical or other effects. The *galvanic* current, on the other hand, has much more extensive properties. In addition to being a nerve-stimulant, it causes contraction of muscular fibre. In small doses, it dilates the bloodvessels, increases the circulation, and raises the temperature. In large doses, it paralyses the vessels, arrests the circulation, and causes destruction of the tissues. It profoundly modifies nutrition, and thereby stimulates into action all the organs. It chemically decomposes all the fluids and solids of the body, and may thus be used as a caustic, and it physically produces such heat as to be efficacious as a cauterising agent.

We shall now consider in detail the effects of each current as applied individually to the different organs and tissues, confining ourselves to those which bear on the present subject of inquiry, namely, the diagnosis of diseases of the nervous system.

The Brain.—The experiments of Hitzig, Ferrier, and others upon animals have shown that faradism, when applied to the surface of the cerebral hemispheres, stimulates the various centres into action which are supposed to originate special functions. Irritation at definite points on the surface of the convolutions, especially round the fissure of Rolando, gives rise to definite movements on the opposite side of the body; whereas in other situations, which have been ascertained not to be motor, the stimulus is not followed by any result. These centres of voluntary motion can, in fact, be stimulated by a weak faradic current into activity, and each thereby performs its own function in the form of special muscular contractions. Faradic irritation of all the motor basal ganglia and motor tracts in the brain causes unilateral movements of all the muscles on the opposite side of the body. Galvanic excitation of similar parts also causes the same muscular contractions, but only at the opening and closing of the current. Although by analogy we may fairly assume that similar phenomena would ensue in

the human subject, I am not aware of this having actually been demonstrated. There is no evidence to show that faradism applied to the exterior of the scalp produces any physiological effects on the brain; but, if a strong current of galvanism be made to pass through the head of an animal, Matteucci found that contractions of all the muscles of the body could be produced; and in man, if a weak current be employed in the same way, definite cerebral symptoms will ensue, such as vertigo, sickness, fainting, convulsions, etc., which, although most marked at the opening and closing of the current, continue all the time it is passing. If the galvanic giddiness be produced by the cathode, or negative pole, being placed on the right side of the head, and the anode, or positive pole, on the left, surrounding objects seem to go from right to left. On opening the current, the body has a tendency to fall towards the anode, and, on closing, towards the cathode. It has been shown by experiment that, when the electrodes are placed on each side of the head, the current of electricity actually passes through the brain. The channel through which this takes place is, according to Erb, directly through the medium of the tissues. Dr. Althaus believes it to occur by reflex action through the fifth pair of nerves, and adduces a case to show that, when those nerves are paralysed, no physiological effects could be produced on the brain by application of electricity to the scalp.

The Spinal Cord.—In animals, a faradic current of sufficient strength applied directly to any part of the cord induces tetanic spasms of all the muscles of the trunk and limbs. This continues during its whole course, which, if continued long enough, will cause gradual diminution of these movements, and ultimate paralysis. If a weak current, sufficiently limited, be applied to the motor tract on one side of the cord, it would produce unilateral contractions; on the opposite side of the body if applied above the medulla, and on the same side if below that centre. Galvanism, in the same way, if applied to any portion of the cord, results in general muscular contractions, but only on opening and closing the current. While the current is passing, there are no apparent movements, but a so-called inhibitory effect is produced, in so far that the cord is rendered insensible to ordinary stimuli; and, while in this condition, if faradism be applied, it will not induce movements of the muscles. This phenomenon is confined to the spinal cord, and does not extend to the nerves: for if, while the current is passing through the former, the latter be stimulated, the muscles readily contract. According to Onimus, a descending current diminishes, and an ascending one augments, the reflex irritation. Faradisation of the cilio-spinal region, or that portion of the cord between the seventh

cervical and the sixth dorsal vertebræ, induces dilatation of the pupil through the influence of the sympathetic nerve, which here takes its origin. Irritation of the genito-spinal ganglion at the level of the fourth and fifth lumbar vetebræ causes contraction of the vasa deferentia, the bladder, and the lower portion of the rectum. There is no evidence that faradism has any influence on the cord of man: but both experiment and observation have demonstrated that distinct physiological effects are induced on that centre by the galvanic current; and this is proved, as pointed out by Erb, by the muscular contractions of the parts supplied from the region of the cord irritated.

Motor Nerves.—On irritating a motor nerve with electricity, the only visible phenomenon which follows is contraction of all the muscles which it supplies. Each shock of the faradic current produces a contraction. This also increases the excitability of the nerve, so that a second shock succeeding the first produces more marked muscular movements. This irritability continues to increase with each shock, and at last reaches a maximum which remains stationary for a time, and then gradually diminishes. If these shocks follow one another in rapid succession, there are no apparent intermissions, one contraction not having relaxed before another has taken place; hence there is tetanus, and this continues all the time the interrupted current is passing. If the application be protracted sufficiently long, the tetanus gradually becomes less marked, and ultimately disappears, the nerve having then lost its power of irritability and excitability. Thus the faradic current in moderate and limited doses stimulates the natural function of nerve, in large or long-continued doses it paralyses it.

The action of the galvanic current on nerve is very different, and much more complicated. When this is applied with moderate power to a nerve, muscular contractions ensue; but these take place only at the moment of making and breaking the current. These are not of a tetanic nature, but short and sudden; and, while the current is passing continuously along the nerve, there are no muscular contractions. These phenomena are influenced by the intensity of the current; the stronger it is, the more energetic the contractions; and, if it be very powerful, there may be tetanic spasms not only on making and breaking, but during its passage along the nerve. The muscular movements vary according to the direction of the current, so that there are four different contractions: (1) the descending closure contraction, (2) the descending opening contraction, (3) the ascending closure contraction, and (4) the ascending opening contraction. These are further modified in strength by the intensity of the current; and the systematic

arrangement of which has given rise to what is termed Pflüger's law of contraction, which is as follows:—

	Descending.		Ascending.	
	Make.	Break.	Make.	Break.
Very weak ...	... c	— ...	... —	—
Weak ...	... c	— ...	... c	—
Moderate ...	... c	c ...	... c	c
Strong ...	... c	— ...	... —	c

It must, however, be observed that this law has been compiled from observation on the lower animals from direct experiment on the dissected-out nerves, to which were applied both poles of the current in different situations. It will subsequently be seen how this table is explained by what has been termed electrotonus, and also that the phenomena are modified when only one pole of the battery is applied to the nerve. Electricity does not cause muscular contractions merely by the power the nerve possesses of carrying its influence to the muscles, but by its specific action in stimulating the nervous elements into functional energy. This is evidenced by the fact that, if a nerve be injured or diseased, no contractions are produced on irritation, although its power of conduction is in no way interfered with. It is also shown by the fact that, the further from the muscle the nerve is irritated, the greater the effect produced; and this is still more increased if a portion of the nerve-centre be included in the circuit.

In man, although there are not the same facilities for applying galvanism directly to the nerves, the same general law of contraction holds good. Here, however, instead of both poles of the battery being placed on any individual nerve, as in the experiments above cited, which give the united effects of the two, a different system is adopted, termed the polar method of investigation. This consists of eliminating the effects of one electrode by placing it on a distant or indifferent part of the body, such as the back or sternum, and applying the other pole to the nerve, and observing its uncomplicated effects. In this way, also, four contractions are produced, namely, (1) C. C. C., or cathodal closure contraction; (2) C. O. C., or cathodal opening contraction; (3) A. C. C., or anodal closure contraction; and (4) A. O. C., or anodal opening contraction. Following this method, it will be found that different degrees of muscular contraction result according to the pole applied to the nerve, as well as to the intensity of the galvanism employed. The cathode always predominates over the anode in its power of inducing functional activity. If a very weak current be used, and both poles in succession be placed upon a nerve, and alternately opened and closed, as the current is gradually increased, the first contraction to appear is the C. C. C. As the

intensity is gradually increased, the C. C. C. augments in force, and, in addition, the A. O. C. is developed. Further increase more fully demonstrates the C. C. C. and A. O. C., and next appears the A. C. C. Finally, a powerful current induces not only C. C. C., A. O. C., and A. C. C., each more powerful than the other according to its order, but ultimately C. O. C. The normal polar reactions of nerve, therefore, occur in the following order: (1) C. C. C., (2) A. O. C., (3) A. C. C., and (4) C. O. C. It must be, however, noted that, in the living human subject, the A. O. C. and A. C. C. are frequently of the same strength, and occur about the same time; and in some cases, even in perfect health, the A. C. C. is more marked than the A. O. C. Under all circumstances, both the A. O. C. and A. C. C. are much inferior in vigour to the C. C. C.

All the phenomena just described take place only when the galvanic current applied to the nerve is closed or opened; that is, when there is any sudden variation in its density. So long as the current is uniform, it may be increased to considerable strength without causing contractions. When galvanism is continuously passing through a nerve, although no muscular movements take place, the condition of the nerve itself is thrown into a peculiar state, termed electrotonus. This electrotonic condition is evidenced by a change in the physiological state of that portion of nerve through which the current passes; and, if this be in the natural direction—that is, upwards—electrotonus is augmented; but if in a contrary direction—namely, downwards—it is diminished. Under these circumstances, the nerve is altered in its power of evolving electricity, in its conductivity, and in its excitability. If a constant current be passing through a portion of nerve, there is a neutral point between the poles which divides it into two areas—namely, one in the neighbourhood of the anode, which is the anelectrotonic area; and the other near the cathode, or the catelectrotonic area. The extent of these two divisions depends on the strength of the current. With one of moderate power, the neutral point is midway between the two poles; with a weak current, it is near the anode, in which case the greater part of the nerve is in a catelectrotonic condition; and, with a strong one, the position is reversed, the neutral point being near the cathode, thus throwing the larger portion of the nerve into anelectrotonus. Now, the excitability and conductivity of a nerve is diminished in the neighbourhood of the anode, and augmented at the cathode: hence, when it is in a catelectrotonic condition, muscular contractions are more readily induced than when it is in a state of anelectrotonus. These changes are most marked at the poles, but extend for some distance on either side. The nerve is excited, and con-

sequently there are muscular contractions when it passes from a lower to a higher state of irritability, as by the appearance of catelectrotonus, and by the disappearance of anelectrotonus. When the reverse takes place, there are no results. A large catelectrotonic area has the power of overcoming the effects of a small anelectrotonic condition; but, on the other hand, a small amount of the former may have no effect in the presence of a large area of the latter. A careful consideration of all these facts on the dissected-out living nerves of animals, and where the poles were directly applied, has given rise to Pflüger's law of contraction already cited.

In man, the same general rules apply, so far as they can be practically demonstrated. Following the polar method, it may be stated generally that the cathode placed over the nerve excites its irritability in the neighbourhood, and that the anode has exactly the opposite effect; while, after what has been already stated, it will be understood that a feeble galvanic current is more stimulating than a powerful one, as the former induces a larger area of catelectrotonus.

It has been already seen that, when faradism is applied to a motor nerve, tetanic contractions take place in the muscles, and remain so long as the current passes. On the other hand, when moderate galvanism is employed, contractions only ensue on opening and closing. This is due to the fact that motor nerves are excited, not so much by the amount of density of the current, as by the suddenness of its variations. With the induced current, these take place rapidly; hence a series of quickly succeeding contractions, resulting in tetanic spasms; while, with the continuous, these changes only occur at opening and closing. If the galvanic current be rapidly interrupted, it has the same effect on the faradic battery in causing tetanus. Galvanism resembles faradism in this respect, that in moderate and limited doses it stimulates, and in large and prolonged doses it paralyses, nerve-function.

Voluntary Muscle.—The faradic and galvanic currents directly applied to a healthy muscle produce precisely the same phenomena as when its nerve is similarly treated. This is due to their effect as the terminal intramuscular branches of the nerves. When these are destroyed by poison or otherwise, leaving the muscular structure healthy, the reactions are very different. The faradic current then applied to the fibres alone produces no contraction, neither does rapidly interrupted galvanism. But, if a continuous current slowly opened and closed be employed, there are muscular movements, and, while the current is passing, there is usually slight tetanic spasm. This demonstrates that the fibre is contractile without nervous influence, although in a different manner. There is another

distinction; for, as we have already seen, when the nerves are intact, the muscular contractions under galvanic irritation are short, sudden, and complete. When nerve-influence is removed, they are more prolonged, more tardy in appearance, and less vigorous in degree. For this reason, if the variations of the electrical current be quickly recurring, the muscles do not contract at all, and this explains the failure in acting to the faradic and rapidly interrupted galvanic influences. To induce contractions, a longer duration of current is necessary, hence their response to the slowly performed opening and closing. Again, a stronger stimulus is necessary to produce the same effect when muscle is directly irritated as compared with nerve. The order of polar reactions is, however, precisely the same as in healthy nerve. To sum up, muscle, when deprived of nerve-influence, differs from the normal condition: 1. In the contractions being slower in appearing, longer in duration, and less vigorous in degree; 2. In requiring a prolonged application of electricity to produce contraction, hence the failure of action under faradism; 3. In requiring a stronger current to produce the same effect; and 4. With galvanism, there are contractions: with faradism, there are none; whereas in a muscle with the nerve intact, there are stronger contractions with the latter than with the former.

Sensory Nerves.—The effects of the faradic current on sensory nerves depend on its strength, the rapidity of the interruptions, and the nature of the electrodes. If of weak intensity, a slight pricking sensation is felt; if powerful, there is intense pain. A rapidly interrupted current has much more effect than a slowly interrupted one, the feeling from a single faradic shock being trifling; but, if rapidly succeeding one another, each is more powerful than its predecessor, from increasing irritation of the nerve, and hence there is greater pain. After a time, a maximum is reached, which remains stationary, after which sensibility diminishes. Gradually a feeling of numbness is produced; and then, although the current is augmented in strength, there is no increase of pain, and ultimately complete anæsthesia may be induced. A moist conductor, by enabling the current to pass through the skin, causes much less pain than a dry one, which encounters great resistance at the more sensitive part of the integument.

The galvanic current causes a sensation of stinging and heat, which continues while the current is passing. If the current be feeble, this is not unpleasant, but, if strong, there is intense burning pain. This is most marked with the cathode, and with a dry electrode. The effects increase the longer the current is applied, because not only does the nerve become more irritable, but the skin becomes a better conductor, owing to

its increasing vascularity. If the current be sufficiently prolonged, the pain diminishes, and numbness, or even anæsthesia, supervenes.

At different parts of the body, the sensibility to the electric stimulus varies greatly; a current, for example, which is unbearable on the face, might not be felt on the sole of the foot. MM. de Watteville and Tschiriew have recently ascertained that "the electrical excitability of the skin, or, rather of the nerves of the skin, is the same at every part of the body." Hence the actual differences of sensibility in various localities must be due to the variety of conducting media which surround the terminal peripheral nerves of the skin, such as thickness and dryness of epidermis, etc.

Both currents produce a peculiar sensation when applied to muscle called electro-muscular sensibility. This is due to contraction, and is proportionate to the strength of the electricity.

To sensory as to motor nerves, both faradism and galvanism act in small and limited doses as stimulants, and in strong and protracted doses as paralysing agents.—*British Medical Journal, Jan.* 10, 1880, *p.* 46.

18.—ON TONGA: A REMEDY FOR NEURALGIA USED BY THE NATIVES OF THE FIJI ISLANDS.

By Dr. SIDNEY RINGER, Professor of Medicine at University College; and Dr. WILLIAM MURRELL, M.R.C.P., Lecturer on Physiology at Westminster School of Medicine, &c.

A few months ago Mr. Ryder, a gentleman residing in Fiji, placed this remedy in our hands with the following account:—It has been used for several centuries by the aborigines of the Fiji Islands. A European, who married the daughter of a chief, learned the secret from his father-in-law, in whose family the knowledge of the composition of this remedy had been an heirloom for upwards of two hundred years. This European gave the drug to Mr. Ryder, who requested us to test its virtues.

This gentleman tells us that the remedy consists of parts of at least two plants, whose botanical relations, however, he does not know. He has returned to Fiji, and intends sending specimens of the plants, that we may learn their natural order, &c. The parts of the plants are broken up into a coarse powder, and then wrapped up in a cover of the inner bark of the cocoa-nut tree. Mr. Ryder gave us the following directions regarding its use:—"The bundle, without being unfastened, to be steeped in half a tumbler of cold water for twenty minutes, then squeeze the liquid from the bundle back again into the tumbler,

and take a claret glass of the infusion three times a day, about half an hour before each meal. Dry the bundle and hang it up in a dry place to prevent its getting mouldy. It will answer for twelve months."

Mr. Ryder sent a large packet of the powder to Messrs. Allen and Hanburys, of Plough Court, City, who have prepared a liquid extract, containing one part of the drug in one part of the extract. Of this Mr. Ryder recommends a drachm three times a day. He and his friends have tried the remedy extensively, and find it most successful. He finds that it generally cures by the second or third day. In eight or ten days the pain may return, when a few additional doses permanently remove the pain. It has been used in Sydney with great success.

We have used this remedy in eight cases of neuralgia; six were promptly cured; one was much improved; in the other, after a week's trial, it failed. We give a short account of these patients.

A woman, aged twenty-three, had suffered for fourteen days from severe neuralgia of the infra-orbital and great occipital nerve. She had four severe paroxysms in the day, lasting from half an hour to an hour and a half. Many of her teeth were bad. Three doses of the extract cured her.

A woman, aged about fifty-five, had suffered from severe neuralgia for a week. The twisting dragging pain affected the supra-orbital branch of the fifth and the great occipital, and was both continuous and paroxysmal. She did not improve, though the infusion from a bag was used for a week. The bag had been used on several occasions, and possibly had become inert.

A woman suffered from neuralgia in the left great occipital nerve. Four half-drachm doses of the liquid extract cured her.

A man, aged twenty-five, had suffered for a fortnight from severe bilateral neuralgia in the temples, in the eyes, and under the eyes. Half a drachm dose of the liquid extract thrice daily cured him in three days.

A woman, aged twenty, for ten days had suffered from severe neuralgia in the first and third branch of the fifth nerve. She had daily about five paroxysms, each lasting from one to two hours. A drachm of the liquid extract thrice daily cured her in three days.

A girl, aged eighteen, suffered from toothache and severe neuralgia along the lower jaw, and in front and behind the ear. Half a drachm of the liquid extract cured the neuralgia in twenty-four hours, but the toothache continued.

A man, aged sixty, who suffered from rather severe bilateral orbital neuralgia, was greatly benefited in three days by a drachm of the extract thrice daily.

A woman, forty years old, had suffered for a month with about six severe paroxysms daily of pain in the second branch of the fifth nerve. The infusion made from a bag cured her in three days.

This remedy, whilst apparently highly useful in neuralgia, produces no toxic symptoms, for we have given two half-ounce doses of the liquid extract at half an hour's interval, and repeated it again in two hours, without producing any effect beyond slight drowsiness. To another person we gave three two-drachm doses at half an hour's interval, and only produced slight drowsiness. These doses did not affect the pupil, nor increase nor lessen the secretion of the mouth or skin, neither did they affect sensation of the skin, supplied by the fifth nerve.

Tonga does not affect the pupil when topically applied to the eye; for Mr. Copley made observations on three people, and applied in each case an aqueous extract (1 in 1) to the eye, repeating the application four times at an interval of fifteen minutes, but the pupil remained unaltered.—*Lancet, March* 6, 1880, *p.* 360.

19.—ON ACONITIA IN NEURALGIA.

By Dr. OULMONT.

The affections known under the general name of neuralgia, which are so painful, and in the majority of cases so difficult to treat, have for a long time been the subject of constant investigation at the hands of a number of experimenters. Clinical experience has recently demonstrated the powerful anti-neuralgic action of crystallised aconitia, and the excellent results which have been obtained by the use of this remedy in the hands of Dr. Oulmont have fully confirmed the opinions in regard to it which have been already advanced. Aconitia, says Dr. Oulmont, is perfectly successful in such forms of facial neuralgia as are not correlated with other lesions, which are not intermittent, and which have not a well marked recurrence; in other words, in those forms to which M. Gubler has applied the term congestive, and which are most frequently caused by exposure to cold. In such cases aconite produces a rapid cure within two or three days. Dr. Oulmont has even seen a case of facial neuralgia of seven days' standing, in which there was no well-marked periodicity, and which had resisted sulphate of quinine, yield instantaneously and permanently to a quarter of a milligram of nitrate of aconite. The results are more marked and rapid in cases of recent neuralgia than in those of long standing. Examples are quoted, however, in which the affection had lasted for periods of one month, two months, and even five years, but

which had yet been cured, the first on the seventh day, the second on the third, and the last in three weeks. Aconitia has also a distinct effect in secondary neuralgia, as, for example, in dental caries, otitis, paraplegia, &c., &c. Acute rheumatic arthritis may be successfully treated with aconitia. In four individuals to whom this remedy was administered in doses, at first of half a milligram per diem, increased gradually to one and a half milligrams, a cure was effected, once in eight days, and once in ten days. The temperature fell from 39° to 36°, and the pulse in proportion. In the other cases the cure was equally obtained, but only on the fifteenth and eighteenth days respectively, whilst the dose was raised to two and a half milligrams. The antipyretic action, however, was equally well marked, whilst the temperature fell on the eighth and ninth days about two degrees. The results obtained by M. Gubler are also noteworthy. The results of four cases were published; in these the patients were treated with hypodermic injections of half a milligram once or twice a day, whilst half a milligram of aconitia, which was gradually increased till this quantity was taken two to four times a day, was administered internally. In these cases a cure was effected upon the sixth, ninth, twelfth, and thirteenth days: in one case there was a slight stiffness of the joints. The influence of the remedy upon the painful symptoms was very rapid upon the second to the fourth days, whilst upon the fever it was slower, though not less marked. The effects are very remarkable according to M. Gubler in cases of neuralgia of the fifth. Dr. Oulmont concludes his work with the statement that aconitia is a remedy of importance, since it acts in a certain definite manner upon the human organism, but from its activity it must only be employed in very small doses and at long intervals. Neuralgia is often accompanied by intermittent symptoms and well-marked periods. In such complications quinine must be employed in addition to aconitia. On account of the energetic action of the remedy the susceptibility of the patient should be tested by administering, in the first place, three pills daily, each containing a fifth of a milligram of crystallised aconitia in addition to five centigrams of pure quinine; one in the morning, one at midday, and one in the evening. If no alleviation of the pain is experienced on the first day, the dose may be cautiously augmented by a pill per diem, until a maximum dose of six in the course of twenty-four hours is attained, and in the majority of cases it will not be necessary to overstep this limit. If slight diarrhœa occurs, the dose must be reduced. Physiological experiments and clinical observations carried on in the Paris hospitals have shown us that these pills have a sedative influence upon the circulatory apparatus through the

vaso-motor nerves, and it is concluded therefore that they are indicated in neuralgia of the fifth, in congestive neuralgia, in painful and inflammatory rheumatic affections, &c.—*Le Progrès Médical; Practitioner, March* 1880, *p.* 205.

DISEASES OF THE ORGANS OF RESPIRATION.

20.—ON THE LOCAL TREATMENT OF PUTRID EXPECTORATION.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

Putrid expectoration is an accompaniment of various chronic affections of the lungs, and especially of bronchial dilatation. In the worst cases of this latter affection the patient is not only a nuisance to himself and his surroundings by the foul odour of his sputa, but the stagnation of the bronchial secretion on which its putridity depends is dangerous to himself in two ways—by the irritation and inflammation it is liable to cause at the seat of its occurrence, and by the infection of healthy portions of lung by inspiration of some of the putrid matter. The uselessness of internal remedies in these cases is well known, and many of them have been and no doubt still are, *opprobria medicinæ*. Treatment by inhalation, especially of the terebinthinates, has been tried, with a certain amount of success more largely on the Continent than in this country, but the method adopted has been faulty. A few drops of the drug have been poured on the surface of hot water and inhaled for a few minutes two or three times a day, and a variety of cumbrous instruments have been suggested for this purpose. Two or three years ago, Dr. W. Roberts, of Manchester, described a simple portable "respirator inhaler," in the form of a metal box perforated in front and behind, and filled loosely with layers of tow on which the inhalation liquid was poured. This inhaler fits over the mouth, and is fixed by elastic bands over the ears like an ordinary respirator. The introduction of this instrument, although the medical profession as a body may have failed to recognise it, was undoubtedly a step in advance. It showed the practicability of a *continuous* method of inhalation. This method has for some time been extensively tried with an inhaler similar in principle to Dr. Roberts', by Dr. H. Curschmann, late of Berlin, and now director of the Hamburg General Hospital, who has done much not only to popularise it, and to do away with objections to its use, but to prove its value in the most positive manner. We propose, therefore, to devote the remainder of this article to an account of his system and its results.

His respirator itself is figured in the Berliner Klinische Wochenschrift, No. 29, page 430, and it is to be obtained from Duntzelt, 22, Schaaren-strasse, Berlin. It is made of vulcanite, and has a rim of soft india-rubber, where it touches the face, to insure close contact and prevent air entering the lungs except through the respirator itself. Dr. Curschmann generally covers both nose and mouth, so that all air which the patient breathes is saturated with the vapour in the inhaler.

The substances used for inhalation, and which are poured on a sponge in the front of the cavity of the respirator, are all well-known drugs—pure oil of turpentine, carbolic acid and thymol, either pure or diluted with from one to three parts alcohol, and creasote. Dr. Curschmann's application of them differs, however, from what most practitioners are accustomed to, in his using them either pure or, if diluted, but very slightly diluted; and yet most careful examinations of the urine after the prolonged inhalation of oil of turpentine never revealed the least renal irritation; nor did the patients complain of any unpleasant symptoms, except occasionally a little oppression of the head and headache. The same is true of the use of undiluted carbolic acid previously liquefied by a gentle heat. If care be taken to wipe the edge of the inhaler frequently where it touches the face, and to anoint the face itself with simple ointment, there is no local soreness; and Dr. Curschmann has never seen any irritating effect produced either on the inside of the mouth or on the larynx by the administration of the vapour of carbolic acid in so concentrated a form; nor has any instance of so-called carbolic "intoxication" occurred in his practice. This statement refers to adults, as he has had scarcely any experience with children. He explains the harmlessness of the pure acid when inspired, first by the small amount of it which evaporates and reaches the lungs at all; and, secondly, by the fact that the large part is, very soon after reaching the dilated bronchi or cavities, expectorated with their secretion, and that the false membrane lining these cavities probably offers considerable resistance to its absorption into the system. Both carbolic acid and thymol evaporate much more freely in alcoholic solution than when pure; and Dr. Curschmann has almost invariably used thymol in this form alone. Alcoholic solutions of carbolic acid are more apt to cause paroxysms of cough than the undiluted acid. More patients, however, object to the use of thymol than of carbolic acid; but the former is, no doubt, safer for children's use than the latter.

Creasote never requires dilution, and on this point we are able to confirm Dr. Curschmann's experience, but it is very important to see that the druggist supplies a pure article. Curschmann prefers creasote in cases where there is a tendency

to hæmoptysis: he finds that it not only has a styptic action and disinfecting properties as powerful as those of carbolic acid, but that its vapour is sedative and allays rather than excites cough. Incidentally he mentions that he has seen benefit result from creasote inhalations in the hæmoptysis of phthisis.

In illustration of the really wonderful effects of the continuous inhalation method in putrid bronchitis, Dr. Curschmann describes in detail two very severe cases of what in ordinary parlance would be roughly called "phthisis." In the first case the right lower lobe was affected, there was dulness to the angle of the scapula behind, with loud bronchial breathing, and abundant moist râles. The expectoration was very abundant, and so fœtid that it was difficult to stay near the patient. Evening temperature 38.6° C.; pulse 100. The patient, a man of thirty-nine, had fallen away very much during his eighteen months' illness, and weighed only fifty-seven killogrammes, though of large build. He was treated with inhalations of pure carbolic acid, at first for two or three hours at a time, and afterwards almost continuously. Within a few days the sputa had almost lost their fœtor; within a month they were absolutely odourless. Simultaneously the temperature became normal, and the physical signs of dulness, etc., as well as the patient's general condition, steadily improved. Before leaving the hospital, and under inhalation treatment alone, he had gained nine and a half kilos, or about twenty pounds in weight. The second case is quite as remarkable. A man of fifty-three, who had been ill some months with symptoms of phthisis, was admitted under Dr. Curschmann's care in November, 1878, with dulness, bronchial breathing, and medium-sized moist râles over the lower half of the right lung posteriorly. At one point percussion was tympanitic, and auscultation revealed signs of a cavity, which was proved to be such by tapping and drawing off some of its fœtid contents. The patient expectorated about a litre of most intolerably putrid secretion in twenty-four hours. His evening temperature was 39° C., his pulse 112, and he suffered from night-sweats. He was treated throughout with almost continuous inhalations, first of oil of turpentine, and then of pure carbolic acid. In three weeks the sputa were quite free from smell, fever and night-sweats had left him, and he only spat up about one-third of the amount on admission. As in the first case, there was an ultimate extraordinary disappearance of the abnormal physical signs, and the patient gained twenty pounds in weight during his scarcely six months' stay in the hospital. Except a little morphia for the cough just at the first, he took no medicine internally—no hypophosphites, no iron, no cod-liver oil. In both cases the

successful result can be attributed to nothing except to the antiseptic treatment—for such it is—by inhalation. We commend the study of the complete history of these cases in the original to our readers, and urge on them a trial of the continuous method of inhalation in suitable cases. We have been surprised to find even physicians with a large *clientèle* of lung patients unaware even of the existence of so simple and valuable an instrument as Dr. W. Roberts' respirator inhaler.—*Medical Times and Gazette, Nov.* 15, 1879, *p.* 559.

21.—ON TEMPERATURE IN PHTHISIS AND SOME OTHER DISEASES.

By Dr. A. T. H. WATERS, F.R.C.P., Physician to and Lecturer on Medicine at, the Liverpool Royal Infirmary.

As a contrast to the oscillations of temperature in acute phthisis, let me call your attention to those of acute capillary bronchitis. Under ordinary circumstances in bronchitis attacking the larger tubes, the temperature does not rise high; but, in capillary bronchitis, the rise is often considerable. It rarely, however, reaches the elevation of acute pneumonic, or even acute tubercular phthisis. There is, in some cases, a difficulty in making a differential diagnosis between acute capillary bronchitis, and acute phthisis. I think the thermometer affords important aid in such cases; and I have been able, by its use—by recognising that the temperature did not reach a great height, although it was sustained for many days at a rather high level—to give a favourable prognosis, although the physical signs were such as might have existed in acute phthisis.

The differential diagnosis between phthisis with cavities, and some cases of bronchitis with dilated bronchi is by no means easy. Without attaching undue importance to thermometric observations, it cannot be doubted that, while the physical signs may be such as to admit of hesitation as to the exact nature of the affection, the thermometer may afford valuable aid. I have referred to the fact that, in capillary bronchitis, I have found that the temperature does not have the afternoon, or evening rise so common in phthisis; on the contrary, the temperature is highest in the morning.

Very important indications of the existence of phthisis are often afforded by the use of the thermometer before any physical signs can be detected. As a remarkable instance of elevation of temperature in such a case, let me call your attention to the following.

M. C. was admitted into the Infirmary under my care on February 12th, 1878. She complained of having had shiverings,

and of feeling generally ill. Her temperature on the day after admission was 101 deg. in the morning, and 102·4 deg. in the evening; and the following morning it had fallen to the normal. A careful examination of the patient was made, and no physical signs of disease could be detected in the lungs or elsewhere. There was neither cough no expectoration. On the evening of this day, the temperature rose to 102·8 deg. at 5 o'clock, and at 9 it had fallen to 100·2 deg. The following day, at 2 p.m., it rose to 103·8 deg., and at 9 p.m., it was down to 100·2 deg.; the next day two rises occurred. These elevations of temperature were accompanied with shiverings, almost approaching to decided rigors, so that the case presented very much the aspect of an irregular quotidian ague, or one of septicæmia.

The diagnosis at which I arrived was, that the case was one of phthisis; but for several days, although the chest was carefully examined from day to day, no physical signs of lung-mischief could be detected. After a time, however, slight crepitation was noticed at the right apex, and the nature of the case became clear. I considered it one of catarrhal pneumonic phthisis of a subacute form.

For some days, the temperature was irregular in its elevations and depressions; but, as the disease became pronounced, the alterations of temperature assumed a form which is, I believe, the most frequent type in phthisis—namely, that the temperature is highest at or about 5 to 6 p.m., that a fall begins from 5 to 7 p.m., that at 9 p.m. there has been a considerable fall, and that a low level is maintained during the night and forenoon.

And it is to this part of the subject that I now wish to call your attention.

In a large number of cases of phthisis—whether acute, subacute, or chronic—I have found that the temperature has been highest at or near 5 o'clock in the afternoon; that, as a rule, there is a decided fall of temperature before 9 o'clock p.m.; and that the temperature is low in the morning. There can be no doubt, I think, that there are different types as regards temperature in phthisical cases. There are some cases in which it is highest late in the evening, 9 o'clock or so. There are other cases in which the temperature is highest in the morning. These are, however, very exceptional cases; indeed, I think this kind of temperature only occurs under exceptional circumstances—circumstances of a passing character. Then, again, there are some cases of phthisis which have two exacerbations, and two remissions in the twenty-four hours, simulating, in this respect, cases of septicæmia. But of a series of cases, now amounting to a large number, and including several thousand separate observations, the main results are as I have stated.

As the result of my observations, it would appear that, whether

phthisis is in an early or a late stage, if active mischief be going on, there are, as a rule, important variations of temperature, and that in a majority of cases—I believe in a very large majority—the highest temperature is marked about or near 5 o'clock p.m., and that there is a very decided remission in the morning. I need not insist on the importance of this fact in reference to the diagnosis of phthisis, and on the great error we might commit in estimating the condition of a patient by a single thermometric observation, made, for instance, in the morning in our own consultation rooms, just at the time when the temperature is lowest. No temperature-observations can be relied on in forming a diagnosis in a case of incipient phthisis, unless made at least twice or thrice a day; and, if only one observation can be made, the worst time for it is the morning.

I cannot stop to consider at any length the question as to how far phthisis may be progressive without there being any elevation of temperature. It is quite certain that some cases are met with where there is little or no elevation, but these are exceptional.

In considering the facts which I have brought under your notice, one cannot help asking whether this particular form of temperature-oscillation is a characteristic of phthisis, or whether it may not be so of all forms of allied diseases. I have endeavoured to ascertain what is the usual course in other forms of tuberculous affections, as of the bones, peritoneum, etc.; but at present my facts are not sufficient to warrant me in coming to any definite conclusion, and I must wait for further inquiries. I will, however, refer to some cases which have a bearing on the subject.

In lumbar abscess and caries of the bones, I have found a more elevated temperature in the evening than in the morning. In one case of pelvic abscess with diseased bone, the temperature was for a few days taken at nine, five, and nine, and it was always highest at five.

In a very interesting case of peritoneal inflammation, which was for a long time very obscure as to its real nature, the observation of the temperature led me to infer that there was some tuberculous condition. The most careful examination of the lungs revealed no disease, and the physical signs observed in the abdomen were such as might be consistent with the formation of a tumour. In the progress of the case, the temperature assumed the type which I have referred to as existing so often in phthisis; and I was led to express an opinion that there was some tuberculous condition present.

In considering these temperature-oscillations, it is impossible not to note that, to a certain extent, they seem to be exaggerations of the normal changes. In the healthy, there is a fall in

the evening and early morning. So in phthisis; but, then, in the healthy, the late morning temperature is fairly high, whilst in phthisis it is often very low.

But I must now pass on to consider another subject. How important becomes the use of the thermometer in the early days of a typhoid fever, when, perhaps, there is no definite symptom to attract attention; but yet the thermometer reveals an abnormal temperature with evening exacerbations and morning remissions, thus enabling us to surmise, if we cannot absolutely diagnose, the existence of the disease. Here, however, I must say that conditions are often met with which must warn us not to attach too much importance to mere temperature. Whilst many a so-called feverish cold has shown itself by its development to be an attack of typhoid, the existence of which might have been feared had the temperature been taken, so, on the other hand, cases are by no means infrequently met with in which no inflammatory conditions can be recognised, where the temperature is so high and the general state such as to give rise to the fear that typhoid may exist, but where, after the increased heat has been maintained for a few days, defervescence occurs, and the real nature of the case becomes apparent.

Amongst the many interesting questions in connection with our subject is: What is the highest temperature which is consistent with the maintenance of life in the human economy? All observations hitherto made tend to show that, when the temperature of the body in any case of pyrexia reaches eight or ten degrees above the normal, there is grave danger unless the heat is speedily reduced. Wünderlich says that the highest recorded temperature in a living man was 112.55 deg. Fahr. Brodie found 111 deg. in a case of injury to the spinal cord. But this temperature was far surpassed by that which is said to have occurred in the remarkable case recorded by Mr. Teale, of Scarborough, in which a lady, who suffered an injury whilst riding, is reported to have had a temperature which on one occasion reached 122 deg.; and Mr. Teale says that, during seven weeks, the temperature never fell below 108 deg., and rarely below 110 deg., and yet the patient recovered.

This case is so remarkable and so exceptional that I think we may set it aside as having no applicability to ordinary cases of fever; and we must, I think, in spite of it, conclude that a temperature of 110 to 112 deg. is not compatible with anything like a prolonged maintenance of life.

Cases of intermittent fever sometimes have a very high temperature. I have myself known a rise to 107 deg.; but Dr. Macintosh states that he has known a rise to 110 deg.; but, then, this high temperature soon subsides, a very rapid defervescence takes place.

In pneumonia, the temperature rarely rises above 106 deg. In a series of one hundred and six cases of the acute form of the disease which have occurred in my hospital practice, there is only one case in which the temperature reached 106 deg. in the axilla, and the case was not fatal. In only a small proportion of cases did the temperature (taken in the axilla) pass beyond 105 deg. The highest recorded temperatures in cases ending favourably that I am acquainted with are 106.7 deg. by Ziemssen, and 107 deg. by Kocker. In fatal cases, a temperature of 108.9 deg., 109.4 deg. has been reached before death.

An examination of the temperature-charts of cases of acute pneumonia affords interesting evidence of the nature of this pneumonic fever. It runs, in some cases, a very definite course; a high temperature is maintained for some time, a so-called crisis comes, and there is a very rapid defervescence. But this is by no means always the case. There are often two, or three decided remissions, followed by exacerbations, before the final defervescence comes.

In cases of septicæmia, the temperature has risen to 109.6 deg., and yet life has been maintained; but apparently this has resulted from the fact that the heat has been quickly subdued by the external application of cold. Again, in typhoid fever, when the temperature has risen to an alarming height, to over 107 deg., a rapid cooling has followed the use of the tepid bath, and life has been apparently thereby saved.

It would be very interesting to consider the causes of increased temperature in pyrexia; but, perhaps, in the present uncertainty of our knowledge, it would not be very profitable. Although increased heat constitutes one of the most prominent symptoms of fever, it is assuredly only one of its factors, or one of its results; and yet it is impossible to shut our eyes to the great importance of carefully attending to it as an indication for treatment. When we see, in cases of septicæmia, of acute rheumatism, of typhoid fever, etc., the temperature rising to 107, 108, 109, or 110 deg.; and when, apparently as the direct result of this increased heat, we witness the gravest disturbances of the functions of the nervous centres, and dissolution threatened; and when, under the influence of external cold, we see the temperature rapidly fall, the coma disappear, the delirium subside, and the patient restored to a condition of comparative comfort and real safety, it is impossible not to come to the conclusion that this high temperature, whatever may be its essential nature, has been, in these cases, a main cause of the alarming symptoms which were present. Were the coma or the delirium due in either case to the poisoned or altered condition of the blood, the mere reduction of heat would scarcely afford the marked relief which we witness. Were the phenomena the

result of some impression on the nervous centres produced by organic change, however slight, it is difficult to understand how this lowering of temperature could give rise to the rapid and striking alterations which are sometimes seen; and thus, I think, in our present state of knowledge, we must admit that temperature plays an important part in the phenomena of the pyrexial state; and, in studying the therapeutics of fever, we may well give a large share of our attention to those measures which have the effect of diminishing heat. This part of the subject is one on which I cannot enter now, although I hope on some future occasion to lay the results of my observations before the Society, and to speak of my experience on the use of cold applied externally, of quinine, of alcohol, of digitalis, of aconite, of salicylic acid, and of free nutrition as temperature-depressors in various forms of fever.

In estimating the rise and fall of temperature in disease, it is obvious that we cannot trust to our unaided senses; and that, in the thermometer, we possess a guide which, if properly used, can scarcely deceive us. The rapid rises of temperature which sometimes occur can only be recognised and appreciated by the application of this instrument of precision; and it cannot be doubted that it has in many cases rendered most valuable service, and been the means of saving life by pointing out the necessity for prompt and decided action.—*British Medical Journal, Dec.* 20, 1879, *p.* 978.

22.—PICROTOXINE AND ITS PROPERTIES.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

The introduction of picrotoxine as a remedy for that distressing night-sweating which frequently exhausts the little remaining strength of the victim of phthisis is worthy of notice.

Picrotoxine is the active principle of cocculus Indicus (*Anamirta cocculus*, the *Menispermum cocculus* of Linnæus), a plant which has been recognised as a medicine since the days of the Arabian physicians, by whom it was described under the name of *maheradsch*. This cocculus was probably first known in Europe as a poison for taking fish, which it first throws into violent irregular motion and then stupifies. All kinds of fish are killed by it; the barbel, it is said, taking the longest to die. Fish are inordinately fond of the berries, and, when rendered helpless by the dose they have taken, they are readily caught. They should be removed from the water as soon as they appear on the surface, and their bellies emptied, or their flesh may become poisonous and cause irritation if eaten. In nearly all civilised countries, the use of cocculus for this pur-

pose is illegal. Cocculus has also been used from an early period by unprincipled brewers, partly for giving beer a due degree of bitterness without the employment of hops, partly to give it "bottom" and render it more intoxicating. In an old treatise on brewing, we find the following instructions. "Three pounds of cocculus Indicus to be added to ten quarters of malt, it giving an inebriating quality which passes for strength of liquor; it also prevents the second fermentation of beer, and the bursting of the bottles in warm climates." It is said to be used by thieves and bad characters for the purpose of drugging their victims.

Picrotoxine is undoubtedly worthy of a trial in obstinate cases of epilepsy, especially when the attacks occur chiefly at night. For chorea, it is recommended by no less an authority than Gubler; and, according to Tschudi and others, it is especially useful in paralysis of the sphincters. It is also employed in various forms of dyspepsia, notably when there is severe epigastric pain aggravated by pressure or by taking food. Dr. Phillips speaks of it as being of singular service when the colon is distended with flatus, and when the bowels are constipated and the motions hard and lumpy. He also recommends it for certain symptoms associated with irregular menstruation. In females of nervous temperament and of thin and delicate fabric of body, the menses are often preceded by paroxysms of colic felt in the hypogastric region, and accompanied by more or less pain in the back and hips. This pain not only precedes the appearance of the catamenia at each period, but accompanies them for the first day or two. They are of a twisting, griping, or colicky character, and are attended by a scanty discharge or a profuse one, in either case somewhat paler than usual, and mixed with clots and shreds of membrane. The administration of two or three drops of a saturated tincture of cocculus three or four times a day prior to the expected flow, and continued during the first two or three days of its progress, will frequently ward off the pains and render the discharge more natural.

Quite recently, Dr. Murrell has introduced picrotoxine as a remedy for the night-sweating of phthisis. He uses a 1 in 240 solution in water, and of this he gives from one to four minims three times a day, the last dose being taken at bedtime, or immediately before the time at which the perspiration usually commences. He has employed this mode of treatment at the Royal Hospital for Diseases of the Chest in twenty cases, with only one failure. The sweating is usually arrested in two or three days, and there is no return for a fortnight or more. The picrotoxine is best given alone, and not in a mixture, and it has been found to succeed after oxide of zinc, belladonna,

Dover's powder, and other remedies have failed. A great advantage of the treatment is that it does not make the skin too dry, but leaves it comfortably moist, whilst not unfrequently atropia seems to parch it up. The aqueous solution is apt to deposit crystals in winter, but it soon clears up on warming.—*British Medical Journal, Jan.* 17, 1880, *p.* 96.

23.—ACCUMULATION OF CERUMEN SIMULATING CHRONIC BRONCHITIS.

By J. RUDD LEESON, Esq., M.B., Late Demonstrator of Anatomy at St. Thomas's Hospital.

Mrs. V. had suffered at intervals of from three to five years from several attacks of acute and subacute bronchitis, from which she recovered, her lungs being left fairly free from physical signs, but which slowly then increased, much to her distress, and defied all treatment. She had a violent morning cough, lasting oftentimes nearly an hour, and terminating with vomiting about half a teaspoonful of glairy mucus. So severe was this morning cough that she dreaded rising, having generally to rest several times during dressing, from exhaustion. The bowels and bladder were relieved almost involuntarily. All sorts of plans, schemes, and medicines were tried, but without success, and I abandoned her case as being a hopeless one of chronic bronchitis.

For about two years she had suffered from increasing deafness, and had almost lost hearing on one side. Lately she began to complain of the good ear becoming affected, and one morning I was called to see her on account of almost complete deafness. On examination, both ears were filled with inspissated cerumen, which, after much syringing and soaking with oil, was removed, and her hearing was completely restored. From that day she has entirely lost her cough and sickness, and appears indeed to be quite another woman. Now this is a most interesting fact, and I venture to bring it before the notice of the medical profession, hoping that perhaps other cases of so-called "chronic bronchitis" may be cured with like rapidity.

Accumulation of cerumen in the ears and chronic bronchitis both chiefly affect those advanced in life. It may be that a connection between the two as cause and effect has hitherto been overlooked. However, in any case of prolonged and troublesome cough which resists the ordinary remedies, I should advise a careful examination to be made with a speculum of the external auditory canal, it being almost needless to say that a considerable accumulation of wax may exist without damaging the hearing to any marked extent.

The explanation of the case is now easy: the cough was

simply a reflex one. All aural surgeons are familiar with the fact that foreign bodies in the ear are frequently attended with a dry cough. Physiologists also know that coughing can be excited by tickling the auditory canal. In this case the stimulus seems to have travelled down the vagus, stopping for some half hour or more at the pulmonary plexuses, and then following the pneumogastric through the diaphragm to the stomach, causing in the first place a flow of mucus, and secondly vomiting.

The anatomy is more difficult. The auricular branch of the pneumogastric nerve supplies, so far as is at present known, the skin *on the back* of the ear, whilst the skin of the external auditory canal is supplied by the auriculo-temporal branch from the third division of the fifth, this being proved not only by dissection, but also, as Mr. Hilton points out in his lectures on Rest and Pain, " by the frequency of earache accompanying toothache and malignant disease of the tongue, and by stiffness of the jaws (the muscles of mastication being supplied by motor branches from the third division of the fifth) accompanying earache, &c.

Now, I am not aware that anatomists describe any communication between the two nerves in question—viz., the auricular branch of the pneumogastric and the auriculo-temporal of the fifth, but I think this case goes a long way to prove that communications between them really do exist, and if so almost certainly at their peripheral distribution. At any rate, the facts are most interesting, and have an important practical bearing, which ought not to be overlooked.—*Lancet*, *Dec.* 6, 1879, *p.* 833.

24.—ON ATMOSPHERIC AND CLIMATIC INFLUENCE IN THE CAUSATION AND CURE OF PULMONARY DISEASES.

By Dr. JOHN C. THOROWGOOD, F.R.C.P., Physician to the City of London Hospital for Diseases of the Chest, Victoria Park, &c.

Those of us who watch the weekly returns of the Registrar-General will not fail to have noticed how soon the effect of a week or two of cold weather is seen in the increased death-rate from such complaints as laryngitis, bronchitis, pneumonia, and pleurisy. That severe cold can greatly intensify the severity of pulmonary affections is shown by Dr. Hjaltelin's account of the pneumonia which was so prevalent and fatal in Iceland during the year 1863. During sixteen years spent in various foreign countries, Hjaltelin never saw any pulmonary inflammation so acute and so rapidly fatal as that which he was called upon to treat in his own native home of Iceland.

In the present day we hear a good deal said of inflammation as being the great destroying agent in pulmonary phthisis or consumption of the lungs. This statement, first put forth by that careful observer the late Dr. Addison, of Guy's Hospital, finds exception in the fact that it is not in the cold countries, where inflammatory affections of the lungs are common, that we meet with most consumption. Dr. Schleisner says (Lancet, 1857), "there are countries where phthisis is unknown, as for instance, Iceland," the very place where pneumonia, as we have just seen, is often rampant. In Finmark no phthisis is seen; and the Swedish physicians maintain that consumption becomes less common as we proceed northwards. Dr. Lombard, of Geneva, says he has never known a case of consumption among the monks of St. Bernard. In the civil population of Great Britain it is found that, in London, the deaths from phthisis are 18 per cent.; in Edinburgh, 11·9; in Leith, 10·3; and in Aberdeen, 6·3. With regard to the prevalence of phthisis in hot countries, I get, from information published by Dr. Pollock, the facts that in the West Indies consumption is acute in form and rapid in progress. In Italy it is a malady of universal prevalence and great intensity, and the Italians regard consumption as a contagious and malignant disease. Without adducing further evidence of the prevalence of consumption in warm climates, and its comparative rarity in those that are cold, I will proceed to consider certain forms of phthisis.

First, we have knowledge enough already acquired to enable us to differentiate phthisis, or consumption, of the lungs into two large groups, and hospital experience appears to me clearly to bear out this distinction. We may recognise as group 1, cases of consumption originating in catarrh and cold, or in some inflammatory attack in the chest produced by cold. This is the kind of consumption prevalent in raw cold climates, common enough in England, and furnishing a large contingent of hospital cases.

Inflammatory action is a very active agent in this form of phthisis, but the disease may run a chronic course; its tendency, under judicious treatment, being oftentimes to more or less fibrosis of lung and arrest of active symptoms.

Patients affected with this catarrhal phthisis derive benefit from going to a mild climate, such as Ventnor or South Devon: and, after what I have just stated, I see nothing paradoxical in Dr. Walshe's words in the last edition of his work on the lungs, page 687: "While Southern Devonia is a favourable resort in certain cases for phthisical Londoners, it seems a very hot-bed of the disease for its own native population." I also understand how it comes about that Mr. A. extols the climate of Davos Platz as having saved his son's life, while Lady B., who

went to Davos with a consumptive daughter, cannot bear to hear the name of the place mentioned.

Group No. 2 comprises that form of disease known as true tubercular consumption. To use the words of Dr. Clifford Allbutt, "the despair of the physician and the terror of the public." This form of consumption comes on insidiously, often from no cold caught, from no privation of food, but simply from some inherent, often hereditary, vice in the system. It is a febrile disease, having much the character of rapid blood-poisoning, it attacks other organs besides the lungs, and lasts only a few months, sometimes only a few weeks. Boudin has described it in its intensity as it prevails in the littoral of Peru and in the West India Islands. According to M. Comeiras, it carries off one-third of the population of Tahiti, prevailing more among women than among men: just the reverse is seen in this country among our cases of catarrhal phthisis. Dr. C. T. Williams was informed by Mr. Busk of the Dreadnought that he made a number of post-mortem examinations on South Sea Islanders who had been attacked with acute tuberculosis on their voyage to London, and in whom the disease proved rapidly fatal.

As a practical inference from the foregoing statements, are we to send all persons who are in no matter what stage of consumption to the coldest place we can find, and expect a wonderful cure to result? By no means. Where the chief agent for mischief that is to be combated is of an inflammatory nature, a mild soothing air is best. Where cough is severe and bronchial spasm frequent, a mild air also is suitable. For a patient recovering from a recent pleurisy or pneumonia, also, I hold that a mild air is eminently beneficial. That cases of catarrhal phthisis arising from cold caught improve wonderfully at Hastings, Ventnor, and Bournemouth, is a matter with most of us of no rare experience. The advantage of such a climate is this. The patient can pass much time out of doors with safety, and can sit with open windows without risk of incurring another attack of bronchitis or pneumonia from the exposure.

Suppose, for the sake of experiment, we wished to make a bronchitis run into a true phthisis, then one way might be to expose the patient freely to cold air while in the weakness of convalescence; but a tolerably prolonged observation among dispensary and hospital patients would make me name, as the most certain way of setting consumption of lungs going, to confine the patient closely to one room, and let him breathe over and over again the same atmosphere, while silence is temporarily imposed on the incessant cough by frequent doses of opium. In this way I am in the habit of saying that consumption may be cultivated and developed out of group 1 into

the more serious form described in group 2, so that it becomes a fearfully destructive malady.

I expect one reason why the Italians speak of phthisis as a contagious disease of great malignity, is that they shut up their consumptive patients within doors slowly to smother in their own exhalations. The tendency of confinement in a close atmosphere to cause blood-spitting and consumption has been convincingly demonstrated by the statistics obtained by Dr. Guy when engaged in investigating the effects of certain trades on the health of those employed.

In the cases of young children who are kept very close in heated rooms, and who are said to be always taking cold, we often see most obstinate cough and catarrh due to the throwing off from the air-passages of a weak poorly-nourished epithelium which in time may choke the air-cells, and so lead to pulmonary consumption. The cure consists in laying aside paregoric and squills while we feed the epithelium with a pure healthy air. Appetite soon returns, and the cough speedily takes to flight.

In removing lingering inflammation after an acute attack on the chest, I have seen excellent results come from a sojourn at Torquay, Ventnor, and similar mild warm health-resorts; but when the disorder has passed from the inflammatory stage to one that involves the general nutrition, and that is marked clinically by softening and breaking down of lung-tissue, with night sweats and copious purulent expectoration, I never saw any good come of a residence in a mild sedative climate.

I cannot from my experience give you one example of a patient well on in true consumption who has found benefit from a relaxing air like that of Torquay. Dr. Symes Thompson once told me of a patient of his who had got well apparently of consumption during a winter abroad, and in whom cough, expectoration, and other morbid symptoms all returned during a few months' sojourn at Torquay.

To cure a softening lung dryness of the air is essential. Whether a dry, cold, pure antiseptic air does really exercise its curative action by checking some septic process that is going on in the lung, or by annihilating certain morbific germs which engender tubercle is not yet proved; though for myself, I confess, the idea is attractive, for I cannot help thinking that germs may be given off in the expirations of people in advanced tubercular consumption that may set up the disease in others who are vulnerable in their pulmonary epithelium, or, in other words, who have a predisposition to consumption.

A French observer, M. Bergeret, believes the expired air of tuberculous patients transports the tuberculous elements from lung to lung. If this be true, all the more need for the tuberculous patient to live in an atmosphere that is hostile to the

propagation of morbific germs. A mild warm air hardly fulfils this indication, and practical experience proves it does not; while practical experience certainly shows that tubercular consumption may be arrested by a clear cold atmosphere at a high altitude.

At Davos Platz and in the upper Engadine, 5,000 to 6,000 ft. above the sea-level, we are assured by the local physicians that among the natives consumption is quite unknown.—*Practitioner, March* 1880, *p.* 161.

25.—ON EMPYEMA.—ANTISEPTIC TREATMENT.

By JOSEPH LISTER, Esq., F.R.S., Professor of Clinical Surgery in King's College, London.

[In a case of empyema most successful results followed a free opening in the chest walls. The pleura ceased to secrete pus, and on the occasion on which the patient was exhibited before Prof. Lister's class, the pleural cavity only contained an ounce of serous fluid.]

It is worth while to remind you of the means by which these results have been obtained. In the first place the skin was well washed with the 1 to 20 watery solution of carbolic acid, which has the power of penetrating the epidermis and hair-follicles and any greasy dirt that there may be on the skin, so that it is quite unnecessary to do as many of our German friends do, wash the skin with soap-and-water and afterwards with sulphuric ether. Give the carbolic lotion a little time to act, and it will be sure to purify the integument.

In the next place, the instruments and hands having been cleansed with the same antiseptic lotion, we made an opening into the pleura under a thoroughly trustworthy carbolic spray. This, perhaps, of all cases of antiseptic surgery, is that which, on the one hand, most tests, and on the other most demonstrates, the efficacy of the spray. It tests it severely, because at every inspiration there is a drawing in of air in some form into the pleural cavity. If the patient is an adult, we can say to him, "Hold your breath," at the moment we make the incision; "Hold your breath," at the moment when we take off the deepest part of the gauze in changing the dressing; but with a young child, who is not amenable to persuasion, we can have no control over the respiration, and consequently during the operation, and also afterwards, at every change of dressing, air is drawn freely into the pleural cavity in some form or other. It is therefore necessary, if the spray is to be effectual, to be particularly careful to have a thoroughly reliable appara-

tus for its production, and that all our manipulations are so conducted that there shall never be a chance of any air other than spray being introduced.

Empyema is, therefore, a case requiring very special care in the use of the spray; but if that care has the effect of preventing putrefaction—and that this has been the case here is amply proved by the fact which you have all had the opportunity of verifying as I handed round the glass—viz., that the serum which has been accumulating for days is, nevertheless, absolutely odourless,—if, I say, we avoid putrefaction completely from first to last, under the circumstances of empyema treated by free drainage, this is a complete proof of the efficacy of the spray. The pleura has been filled again and again in the course of these three weeks with atmosphere in the form of spray, and there can be no reasonable doubt that if ordinary unpurified air had entered in the same manner, carrying in its dust through the free opening, putrefaction would have occurred within the pleura. We have here, therefore, as good evidence as any experiment in a laboratory could afford of the power of the spray to correct the septic property of the atmosphere, or, in other words, to destroy the energy of the septic ferments which the atmosphere contains.

In the next place we have used carbolic gauze as a dressing. We have improved upon this gauze of late. The proportions we used of the ingredients were originally one part of carbolic acid to five of common resin, and seven of paraffin, the paraffin being added to prevent undue adhesiveness. We have now changed these proportions to one of carbolic acid, four of resin, and four of paraffin. By that means we have in the first place a gauze dressing with half as much carbolic acid again in it. It turns out that this is not too irritating, and therefore that is a great improvement; for the gauze is of course more efficacious antiseptically in proportion as it has more carbolic acid in it. From the diminution of the paraffin we have a little more adhesiveness, but that, I think, is positively an advantage. We do not find it causes any serious inconvenience, and, on the other hand, it tends to prevent the disposition of dressings to slip upon the skin; it helps to keep them more securely in place. And then I may mention as a secondary, though not insignificant advantage, that though we thus increase so considerably the proportion of the carbolic acid, which is the most expensive ingredient, we have not added to the cost of the materials as a whole, because pure paraffin is so much more expensive than resin, that by diminishing the proportion of paraffin to the resin, we have cheapened the article more than we have enhanced its price by

increasing the quantity of carbolic acid, so that the gauze is rendered slightly cheaper by this alteration of our proportions.

The gauze has been employed in the usual manner in eight layers, with a piece of mackintosh under the outer fold; but in a case like this we have also used the gauze in large amount in the form of loose-folded pieces underneath. In the earlier stages this is of great importance, because the flow of serum that takes place from the unsupported pleura in the earlier stages is exceedingly copious; therefore we have made the mass of gauze more than usual. In the adult it becomes necessary to change the dressings twice a day in these cases, but in this child the discharge being of course much less, once in the twenty-four hours proved sufficient.

Then as to the keeping of this gauze dressing in position, you saw the elastic bandage round the edges of the dressing applied just sufficiently to bring the elasticity of the band into play, and so ensure that the edge of the dressing is always in contact with the skin, and thus, in spite of the respiratory movements, we get accurate apposition of the dressings, a point of great value with reference to the security of our results.

With regard to the kind of tube employed for drainage, for the first few days we had one of caoutchouc, as usual. There was only this difference as compared with its use in ordinary wounds: that into each of the two loops of silk connected with its orifice we introduced a substantial mass of gauze (soaked, like the deepest pieces of loose gauze, in the carbolic lotion), to make sure that the tube should not be sucked into the pleural cavity, as it might be if there were only the silk thread applied to the skin. If the drainage tube were to enter the pleural cavity we should probably never be able to get it out again; and although it might perhaps lie there without causing disturbance, the occurrence is certainly one to be avoided. But in the course of a few days we found, as is usual in empyema treated by drainage, that the tendency of the thorax to contract on that side produced approximation of the ribs, so as to compress the india-rubber tube and interfere with the free flow of the discharge through it. We therefore substituted the metallic tube, about three-quarters of an inch long, long enough to go thoroughly into the pleural cavity, with a collar of metal to prevent it from slipping in, the tube being rounded at the end, with holes at the sides. It is sometimes the practice in Germany to cut away a piece of the rib in these cases to ensure free drainage. This I have not found to be at all necessary: the metallic tube, if used early enough before the ribs have got into contact, answers quite satisfactorily.—*Lancet, Dec.* 20, 1879, *p.* 901.

26.—A CONTRIBUTION TO THE THERAPEUTICS OF STARCH DIGESTION.

By Dr. WILLIAM ROBERTS, F.R.S., Professor of Clinical Medicine at Owens College, and Physician to the Manchester Infirmary.

The digestion of starch consists, as is well known, in its conversion into sugar and dextrine. By this change starch becomes soluble and diffusible, and thereby adapted for absorption from the alimentary canal. It is further known that this conversion is effected partly by the saliva and partly by the pancreatic juice, and that the actual agent of the transformation is a special ferment contained in these secretions. As regards saliva, the ferment goes by the name of ptyalin, and is held to be identical with the diastase of malt. The corresponding pancreatic ferment has not received a distinctive name; indeed, until recently it was not thought to be a separate body; it was rather supposed that the pancreatic juice contained a single ferment which possessed manifold powers, and was able at the same time to peptonize proteids, to emulsify fats, and to convert starch into sugar. It is now, however, ascertained that these several powers correspond to separate ferments. The question has, as yet, been scarcely raised as to whether the diastatic agents of the saliva, of the pancreas, and of malts, are one and the same ferment, or whether there are not more than one—perhaps several—modifications, all capable of doing similar work. It may be regarded as probable, in view of certain recent observations, that the latter supposition will turn out to be correct.

As regards the pancreas, I have obtained evidence, which I need not here particularise, that its diastatic agent is a distinct body from that of saliva and malt. Until further inquiry it will be convenient to use the word "diastase" as a common term, signifying an anylolytic ferment—that is to say, a ferment having the power of resolving the starch molecule into more diffusible bodies of the sugar and dextrine class; and we may conveniently designate diastatic agents according to their local source as malt diastase, salivary diastase, pancreatic diastase, and so forth.

Before diastase can exercise its power the cellulose investment of the starch granule must be ruptured. This is accomplished for the human subject by the art of cooking. In boiling and baking the starch-granules break up under the combined influence of heat and moisture, and the liberated starch swells out enormously by imbibition of water into a mucilage or jelly-like

mass. Unless starch is previously changed into this gelatinous state it is acted on very slowly by the diastatic ferment. It is a matter of capital importance, therefore, in the sick-room, to make sure that gruels, puddings, and other farinaceous dishes prepared for the invalid are thoroughly cooked.

Having premised so much, I pass to the consideration of the means we possess of artificially aiding the digestion of starch in cases where that process is defective from deficient diastatic power in the saliva and pancreatic juice. Unfortunately our knowledge of the defect which it is proposed to remedy is very incomplete. Something, however, we do know with fair precision. We know that in infants under three or four months old the saliva has but a feeble diastatic power. I have further ascertained the significant fact that the pancreas of suckling calves is inert on starch, and the inference is strong that the pancreas of the infant at the breast is in the same predicament. It is not probable that the diastatic power is wholly wanting in sucklings—seeing how widely this power is distributed; but it appears certain that it does not exist in sufficient amount to be available for the digestion of sensible quantities of starchy food. This being so, it is obvious that farinaceous articles are unfit food for young infants unless artificial means are used to assist their digestion. With regard to older children and adults we possess very little exact knowledge respecting defective secretion of salivary diastase, and none at all respecting defective secretion of pancreatic diastase. We may nevertheless pretty safely conclude that whenever the mouth is dry there is diminished supply of salivary diastase. In the febrile state, in advanced stages of most organic diseases, after alcoholic excesses, and in a multitude of morbid conditions of various kinds, the mouth is dry and the saliva is scanty; and it is reasonable to infer that we shall be doing an important service to our patients so suffering by remedying this defect by artificial means.

In malted barley we have at command an unlimited supply of diastatic power, and it is not surprising that many eyes have been turned in this direction, and that many efforts have been made to utilise this resource as a means of assisting the digestion of starchy food when the supply of natural diastase is deficient. Of late renewed interest in the subject has been created by the introduction into pharmacy of a new class of malt preparations, which, under the name of "malt-extracts," are now challenging the attention both of the profession and the public in a very prominent manner. These articles are now manufactured on an enormous scale.

Malt-Infusion.—The cold-water infusion of malt is an energetic diastatic agent, and trials of its action on various

farinaceous gruels have led me to believe that it may with advantage be added to our list of malt-preparations. I have adopted the following method and proportions as yielding a pharmaceutical preparation of convenient standard strength. Three ounces (or three piled-up tablespoonfuls) of crushed malt are thoroughly well mixed in a suitable vessel with half a pint of cold water. The mixture is allowed to stand overnight —that is to say for twelve or fifteen hours. It is then filtered through paper until it comes through perfectly bright. The above quantities yield about seven ounces of product. Malt-infusion thus prepared has a light-brown colour like sherry, a faint sweetish taste, and the odour of beer-wort. It is nearly neutral in reaction, and its sp. gr. is about 1025. It is rendered cloudy by nitric acid, showing that it contains albumen. Its chief solid constituent is maltose, and it is rich in diastase. I was surprised to find that the action on starch of the above standard malt-infusion was quite as powerful as that of average specimens of malt-extracts. This shows that in spite of all the precautions used in the manufacture of malt-extract, a considerable amount of the unstable diastase is rendered inert in the process of concentration. Malt-infusion is very prone to fermentation, and it must either be prepared fresh for each day's consumption, or means must be used to preserve it from decomposition. This may be easily accomplished by adding a few drops of chloroform to the infusion, and keeping it in a well-corked bottle. If the taste and odour of chloroform thus communicated to the infusion be objected to, it may be obviated by pouring out the intended dose into a wine-glass two or three hours beforehand. The chloroform flies off in vapour, and the infusion is restored to its original taste and smell.

Preparations of malt may be utilised for their diastatic property in two ways: either they may be administered *with a meal*, so as to perform their mission during, and for a short period after, the ingestion of the food, or they may be *mixed with the food beforehand*, with a view to predigest the starchy matters before they enter the mouth. I shall consider these two methods separately.

Administration of Malt-preparations with Food.—The labels on the malt-extract bottles direct one or two dessert-spoonfuls to be taken *after* a meal. This is obviously a bad mode of utilising the diastatic powers of the preparation. A meal is often a long affair; the earlier portions may be in full digestion before the final portions have passed the mouth, and it must be borne in mind that diastase is rendered inert by the acid of the stomach, and that as soon as the meal is penetrated by the acid gastric juice the conversion of starch is arrested, and no further progress is made therein until it starts afresh in the small intes-

tine under the influence of the pancreatic juice. When a malt-preparation is administered after a meal the dose is not properly mingled with the food, nor is it mingled at the right time. A better plan is to direct the patient to sip his dose of malt-extract, or of malt-infusion, as the case may be, during the progress of the meal. The object in view is to supplement the action of the saliva, and the artificial substitute should, like the natural article, be mingled with the food in the mouth so that it may have due opportunity for the performance of its destined work before its energies are checked by the rising tide of the gastric acid. Malt-extract is taken in doses of one or two dessert-spoonfuls diluted with water or milk. Malt-infusion may be taken in similar quantities and in the same way; or it may be added to the beverage which happens to be used with the meal, for it has little taste of its own. I may mention that malt-extracts, from their syrup-like consistence and flavour, are suitable for spreading on bread or toast, or for sweetening any kind of farinaceous pudding, gruel, or porridge. An effectual commingling of the ferment with the food is thus ensured.

The mode of proceeding with the malt-infusion is as follows:—A suitable gruel is prepared from wheat, or other flour, or from oatmeal, groats, pearl-barley, arrowroot, or any other farina. The gruel may be made with water alone, or, as is more usual, with the addition of milk or some kind of meat broth. In either case the gruel should be well boiled, and strained to separate the lumps. When the gruel or broth is cold, or at least sufficiently cool to be tolerated in the mouth, the malt-infusion is added. One tablespoonful (well mixed therewith) is sufficient to digest half a pint of gruel. The action is very rapid; in a few minutes the gruel becomes thin from the conversion of the starch. When this point is reached the food is ready for use. The only precaution to be observed in the process is to make sure that the gruel is at least sufficiently cool to be borne in the mouth before the malt-infusion is added. It is not of the least consequence if the temperature be below this point, for the transformation goes on just as well when the gruel is cold as when it is warm; whereas too high a temperature endangers the activity of the ferment, which is rendered inert at and above 157° F. The product of the action of malt on starch is not cane-sugar nor grape-sugar, but maltose, and maltose has very little sweetening power. This is the reason why gruel thus digested suffers little change of taste, so little, indeed, that the addition of it to milk or broth produces scarcely any appreciable alteration of flavour.

Malt-extracts are less suited than malt-infusion for the pre-digestion of farinaceous aliments. Malt-extracts have a somewhat strong sweetening power, and they communicate a

dingy brown colour to the food; whereas the infusion adds neither adventitious taste nor colour. Malt-infusion has also an enormous advantage in point of price. Malt-extracts cost three shillings a pound—the infusion can be made for three farthings a pound. The trials I have made, in actual practice, of food thus concocted, have been highly satisfactory.

I cannot take leave of this subject without adverting to the extraordinary power of extract of pancreas (liquor pancreaticus) as a diastatic agent. In this respect it far transcends any malt-extract. A sample, made by infusing one part of fresh pancreas with seven parts of water, was found, on trial, to possess fully twenty times the diastatic power of the above described standard malt-infusion. Pancreatic extract must therefore be considered as taking the first rank among the available artificial aids to the digestion of starch. It may be administered for that purpose exactly in the same way as the malt-infusion, but in reduced doses. As, however, extract of pancreas has other and probably more important uses as an artificial aid to the digestion of proteid substances, I will content myself here with this brief allusion, and refer the reader who wishes for further information to a paper on digestive ferments published by the writer in the British Medical Journal for 1st and 8th of last November.—*Practitioner, Dec.* 1879, *p.* 401.

27.—ON THE ADMINISTRATION OF ENEMATA IN INTESTINAL OBSTRUCTION.

By GEORGE BROWN, Esq., M.R.C.S., London.

My chief object in writing this paper is to enforce the opinion that, where the administration of enemata for intestinal obstruction is necessary, it is the duty of the medical attendant to perform the operation himself, instead of entrusting the work to a nurse or to the friends of the patients, as is commonly done. When I speak of "intestinal obstruction" I do not include in that category the numerous cases of loaded rectum which come under our notice. In such a condition it is quite possible that any ancient dame would prove equal to the emergencies of the occasion. But the case is totally different when the seat of obstruction is in the colon, the cæcum, or some part of the small intestines. In such cases I would say that the medical attendant fails in his duty to his patient if he does not himself administer the enema, or at least superintend its administration. Moreover, I believe, were this generally done, life would not unfrequently be saved, and often operative measures be rendered unnecessary. How often are cases reported at the societies and in our journals in which the remark occurs, "An enema was now administered; no effect!" Such

enema probably consisted of a few ounces of warm gruel with a little turpentine, which was injected into the rectum by a nurse (who, it is possible, felt disgusted at being called upon to officiate upon the occasion), and which returned immediately without causing an evacuation.

In administering enemata in intestinal obstruction it is not sufficient to give one injection, and, as soon as that returns, to desist. The method I adopt is to fill the intestines, as far as possible, with the fluid used as an injection again and again, even to half a dozen times if necessary, until a satisfactory action of the bowels has been induced. By following this plan I have successfully treated several cases of intestinal obstruction accompanied with symptoms of an alarming character.

A short time ago I was called to a gentleman, eighty-four years of age, whose bowels had not acted for some three or four days, notwithstanding frequent doses of aperient medicines. He was suffering from great pain and extreme exhaustion. On examination I found the rectum empty. The case being urgent, I at once gave him an enema of warm water and soap. At first I was able to pass into the bowels less than eight ounces of fluid, which returned almost immediately, scarcely discoloured. At the next attempt I was able to pass into the bowels twelve ounces of fluid, which was retained several minutes. When it returned some scybalæ were passed with it. I then gave a third enema, this time injecting nearly a pint and a half. This had the effect of causing an immediate and copious evacuation, and an hour or so after, the patient had a second motion, with entire relief to the symptoms.

Recently I was called, late at night, during the temporary absence of the medical man in attendance, to see a youth who had been ill for some weeks, the primary disease being, I ascertained, acute peritonitis. The symptoms he was then suffering from were those of stoppage of the bowels. There was intense pain and violent retching and vomiting. I ordered an enema, when the nurse (said to be a trained nurse) replied that she had just administered one, but without result. She had simply injected a few ounces of fluid into the rectum, which was immediately returned without any effect upon the bowels. The nurse procured a plentiful supply of warm water, and I spent nearly an hour with the patient, giving him a succession of enemata, the fluid injected amounting in the aggregate to several quarts. The fæcal matter evacuated was not large in quantity, but it was hard and of a very offensive odour. I left him free from pain, with nearly three pints of warm water in his intestines. This passed shortly afterwards, together with a large scybala. This appears to have been a case in which,

through peritonitis, the intestines had become matted together, almost entirely preventing the natural vermicular action. To give purgatives in such cases does more harm than good. Enemata given in the manner indicated above is, I believe, the only treatment likely to be attended with benefit. I might multiply cases did space permit, but will only give the following, which strikingly shows the good result of administering repeated copious enemata.

On Sunday evening, December 1, 1878, a young man, twenty-one years of age, a clerk in a merchant's office, called at my surgery for advice. He stated that for some days he had felt very uncomfortable about the stomach and bowels, and, being generally constipated, considered that he wanted a good purging. He therefore took three "antibilious pills" on the Saturday night, and an ounce of castor oil the next morning, which had the effect of sending him to stool, according to his own account (about which doubtless there was some exaggeration), thirty or forty times. But although the bowels had been freely acted upon, the pain and discomfort continued unabated. He now complained of pain and tenderness of bowels, nausea, thirst, inability to take food, and prostration. There was considerable pallor and anxious expression of countenance; dry, furred tongue; temperature 100·4°; pulse 106. On examination, I found the right iliac fossa filled by a tumour, which extended from Poupart's ligament nearly to the anterior superior spine of the ilium, and internally to within an inch and a half of the middle line. It was hard, but very tender to the touch, and distinctly prominent. There was no fluctuation, throbbing, or bruit. As to the nature of the case, I was unable to pronounce a diagnosis, but, seeing that the bowels had been so well relieved, I thought the tumour was due to inflammation of the vermiform appendix and cæcum, with commencing abscess, in the cellular tissue about these viscera, or to scrofulous or cancerous enlargement of the lumbar glands. I decided to keep the case under observation, merely ordering rest in bed, liquid diet, fomentations over the painful part, and a saline draught with quarter-grain doses of morphia every three hours. Next morning (December 2) he was in less pain; temperature had fallen to 99°, and pulse to 98. In the evening, however, the temperature rose to 101°, and the pulse to 112. Discontinued the morphia, and ordered a saline mild aperient mixture every four hours, and a chloral draught at bedtime.

December 3.—Had a bad night. Pain and tenderness of bowels unrelieved. Temperature 99·2°; pulse 100. Evening temperature 102·2°; pulse 116. No action of bowels.

4th. To-day the temperature fell to 99·8°, and the pulse to 100. In other respects there was no marked alteration in the

patient's condition. By this time I began to suspect that the tumour was due to impacted fæces in the colon, and only required a patient use of enemata to effect a cure, but, before carrying out this form of treatment, I consulted my friend, Mr. S. Gardner, who, after examining the patient, concurred in this view of the case; and in the afternoon of the same day I began the administration of enemata, giving three injections of warm water and soap within an hour. At the first trial I injected about twelve ounces, which returned scarcely discoloured; on the second occasion I injected about a pint; and on the third attempt I succeeded in injecting more than a pint and a half of fluid, which was retained several minutes. During the administration of this injection, and whilst the fluid was in the intestines, the patient promoted its passage up the canal by friction with the hand over the abdomen, moving the hand from left to right in the course of the colon. We also kneaded the abdomen over the tumour. With the discharge of this enema a quantity of hard scybalæ came away, and a few hours after he had a further copious evacuation, which, he said, was of an extremely fœtid odour. At night his temperature was 103°, and pulse 110, but he was comparatively free from pain. The tumour was diminished in size, but was still very obvious.

Next morning (December 5) I administered two enemata in quick succession, which caused other evacuations. The temperature was then normal, and so continued to the end of the illness. At bedtime he took a strong dose of calomel and podophyllin in pill, which acted four times before my morning visit. He was then much better, but I considered it advisable to give two enemata. Nothing but liquid stool passed. Next day he was able to get up and walk about. On the 8th I administered a final enema, as he had arranged to go into the country to his friends on the 9th. He retained a quart of fluid without difficulty, and after kneading the bowels over the right iliac fossa he had another evacuation of scybalæ. When he returned from the country, about a fortnight afterwards, all trace of the tumour had disappeared.

In giving enemata in this manner, only warm water and soap are necessary, as turpentine, &c., are likely to cause action of the bowels before sufficient time for the softening of the fæces has elapsed. Care should be taken also that the water is not too warm, or it will be speedily expelled from the bowels. I generally commence with the water 96°, and increase the temperature gradually in the later injections to 105° or 106°. The enema should be injected slowly and without the use of force.

In all cases of intestinal obstruction I would recommend this

mode of treatment to be carefully carried out before operative measures are resorted to.—*Medical Times and Gazette, Sept.* 20, 1879, *p.* 338.

28.—ON RETENTION OF FÆCES.

By Dr. J. MATTHEWS DUNCAN, LL.D., Physician-Accoucheur and Lecturer on Midwifery at St. Bartholomew's Hospital.

[One of the most common causes of retention of fæces is a scybalum.]

A scybalum is a rounded or oval mass of fæces, of larger or smaller dimensions, the size of a hazle-nut or of a hen's egg, or even larger, which has been long retained, becomes partly decolorised, hardened (especially on its surface), and which is sometimes encrusted with salts of lime, producing a rough shell somewhat resembling that of a hen's egg. Such scybala may be found in any part of the great gut. They are not always the cause of retention of fæces. The further up the gut they occur, the more likely they are to meet with fæces which are fluid enough to pass easily by the side of the scybalum, and then they do little harm. A case occurred in my practice, not long ago, of a woman dying slowly from malignant disease of the peritoneum. She was examined by myself and several physicians, who correctly diagnosed the disease, but incorrectly diagnosed two egg-like tumours which were for many months felt in the belly floating in the ascitic fluid which was one of the indications of her disease. These were supposed to be malignant masses. After death they were found to be scybala in the transverse colon, which were causing no irritation, and, apparently giving the woman no trouble. The mistake, you see, was one of very little importance, but it is well to call attention to it, because under other circumstances the mistake might have been of the greatest importance. If we had founded on these scybala as evidences of malignant disease, we should have been misled; but we did not. We thought these were merely part of a disease which we diagnosed upon other grounds.

When a scybalum is low down, especially if it is in the rectum, the fæces are very likely to be obstructed and retained. In this case you not only have retention *of* a scybalum, but you have retention *by* a scybalum. Then the woman's only chance of having her bowels evacuated, if the scybalum persists, is in the motion being fluid and passing by the side of the scybalum. A scybalum in the rectum, obstructing the fæces in this way, is often called a scybalum acting like a ball-valve; but that is an example of *lucus a non lucendo*, for it is not acting as a ball-valve. That is why it is called a ball-

valve obstruction sometimes—because it does not obstruct! the fluid fæces do get past it. Solid fæces are undoubtedly often obstructed by it; but it is only when the fæces are nearly solid that it produces ulterior consequences. It may permit passage of fluid fæces copiously, and yet be causing retention of the nearly solid fæces.

It is in this retention of fæces by a rectal scybalum that you have the best example of the disease that we are considering to-day. A woman having any form of retention of fæces may be truly described, in many cases, as being constantly purged; and in this way the practitioner is put off his guard. A woman having the greatest and most dangerous retention of fæces may be incessantly defæcating, and even in very fair quantity, and even nearly solid fæces, as one of my cases for this day demonstrates. You can see very strong analogy between this and the retention of urine in the bladder, which I was speaking of in my last lecture. In that disease a woman may pass urine frequently, and in large quantities; and yet there is retention. So it may be in the case of retention of fæces. In a case of retention of fæces by a scybalum in the rectum, the accumulation of fæces takes place first in the rectum, and it produces at last a tumour, which can be felt gradually forming in the left iliac region. This tumour presents generally little or even no resonance, is densely hard; and I have seen it repeatedly taken for malignant disease. I will describe to you the features of a case of this kind from the last example of it which came under my own observation. I was asked to go to the country, as a mere form, to see a lady who was dying from malignant disease of her rectum. She was described as being so far advanced in the disease that the malignant tumour could be felt in the left iliac region. Her torments with pain and tenesmus were agonising. She was never off the chamber-pot, and she was incessantly passing thin yellow fæces. This was simply a case of accumulated fæces by a scybalum in the rectum. With my fingers I dug out of her a very large potful of decolourised fæces of a disgusting odour—not the ordinary fæculent odour; and the tumour disappeared and the woman was cured. Now, you can easily understand how natural it was to fall into the mistake that I have described; and a case, which I shall presently read to you, will further impress on you the danger of judging that there is no retention because a woman is defæcating, even frequently. This has a very important practical bearing not only on the diagnosis and treatment generally, but it has a very important practical bearing on the question of colotomy. You are not to suppose that colotomy is necessarily excluded from consideration because the fæces are passing. The reten-

tion of fæces may be going on to a dangerous and even fatal amount, although fæces are passing; and colotomy may be imperatively demanded.—*Medical Times and Gazette, Nov.* 8, 1879, *p.* 521.

29.—A CASE OF RETENTION OF FÆCES FOR TWELVE MONTHS.

By Dr. JAMES DUNLOP, Professor of Surgery, Anderson's College, and Surgeon to the Royal Infirmary, Glasgow.

[A lady fifty-four years of age had been in bad health for twelve months owing to obstinate constipation.]

I introduced my oiled finger cautiously and tenderly into the rectum, half expecting that I should find a stricture due to a mass of malignant disease. Instead of a diseased mass my finger came in contact with a round smooth tumour about the size of a small cricket-ball. It was quite movable, and I could turn it round and round in a large pouch in the rectum. I noticed that as I moved the mass about a quantity of thin greenish fæculent matter with a most offensive odour escaped. As she was suffering greatly, and very nervous, I had to content myself at this visit with the examination only. I was, however, satisfied that the case was not one of malignant disease of the rectum, but only a scybalum which was lying in a large pouch of the rectum just above the anus, and that while it was retained it obstructed the solid portion of the fæces, only permitting the fluids to pass from time to time along its sides. On the following day, with my fingers, aided by the handle of a spoon, I broke down the mass and removed it in pieces.

On the removal of the scybalum, which was dry, hard, and greenish in colour, I washed out the rectal pouch with tepid water, and to my surprise there was ejected quite suddenly a large quantity of ill-conditioned fæces which had long been pent up in the descending colon. Since then the lady has regained strength, and is now quite well.—*Medical Times and Gazette, Dec.* 27, 1879, *p.* 714.

30.—ON THE TREATMENT OF CHRONIC DYSENTERY.

By Dr. C. H. RALFE, F.R.C.P., Senior Physician to the Seamen's Hospital, and Teacher of Physiological Chemistry, St. George's Hospital.

Since active and special treatment has been abandoned, and more reliance placed on the good effects of rest and diet, the experience of the disease as met with at the Seamen's Hospital has certainly become more hopeful. In my treatment of the disease at the hospital I have benefited by the experience pre-

viously gained by my colleagues, Dr. Ward and Dr. Harry Leach, and have abstained from active and special treatment, except to meet occasional and urgent symptoms. Instead, however, of administering an occasional dose of castor oil, I have *systematised* its administration by giving it on stated days at least twice in the week, with the view not merely of removing retained scybalous fæcal matter and irritating discharges, but to prevent their accumulation by sweeping out the bowels at regular and stated intervals.

The castor-oil treatment may be advantageously employed in all stages of the disease—either at an early period, when the disease is subacute and the ulceration still extending, or when the process of repair is going on, and we are dealing merely with an ulcerated condition of the bowel; or when the ulcers have healed, and there remains a thickened and contracted condition of bowel, caused by cicatrisation, with a mucous surface extremely liable to catarrh. It is apparently beneficial by causing a soft non-irritating flux from the small intestines, which sweeps before it scybala, decomposing shreds of mucous membrane, or food, and offensive discharges accumulated in the lower bowel. It is not improbable, in addition to this mechanical action, that the flux from the small intestine may have some remedial action on the ulcerating surface analogous to that said to be produced on the inflamed mucous membrane of the bladder by the injection of normal urine. In the later stages of the disease, when cicatrisation is completed, or nearly so, castor oil relieves the constipation, the consequence of the thickened coat and diminished calibre of the bowel, and it also appears to diminish the tendency to catarrhal relapses. With respect to the dose, in twenty-one cases two drachms of the oil were administered every alternate night. Of these, six were discharged cured. In the remaining seventeen cases the oil was given twice a week in half-ounce doses, and the number of cases discharged cured amounted to eleven. Each dose of castor oil was guarded by the addition of eight drops of laudanum. The administration of castor oil does not usually lead to an immediate diminution of the number of stools, its first effect being to improve their character. By the end of the first or second week the stools become more feculent and less offensive, and the tormina and tenesmus are less urgent. The stools then gradually fall in number, preserving often a regular gradation, interrupted, however, by repeated relapses, each relapse, if treated promptly with ipecacuanha, being more manageable and of shorter duration.

When castor oil alone was relied on the results were not so satisfactory as when either a mixture of bismuth or hæmatoxylon was given at the same time, or at some period during treatment.

Indeed, the most satisfactory results were obtained with half an ounce of castor oil administered twice a week, and a mixture of hæmatoxylon given three times a day. I cannot, however, at present come to any decision with regard to the respective merits of bismuth and hæmatoxylon. In the majority of the cases treated with bismuth the castor oil was administered in two-drachm doses, repeated every other night, and the cases treated with hæmatoxylon with half-ounce doses of oil only twice a week. The more favourable result obtained with the latter treatment may be due to the larger but less frequent dose of oil. Both have a different action, bismuth diminishing the catarrh of the mucous membranes in the neighbourhood of the ulceration; hæmatoxylon by restraining undue peristaltic action throughout the intestinal canal, allowing digestion to be more completely performed, and in this way improving the general nutrition, and diminishing the irritation of the ulcerated surfaces by lessening the passage over them of undigested matters. In four cases turpentine was administered in conjunction with the oil. Turpentine seems to be indicated where we have a large ulcerated surface, and the discharge is offensive. A considerable number of cases treated solely with turpentine were collected for me by my friend, Dr. Andrew Duncan, when house-physician at Greenwich, the notes of which I have unfortunately mislaid, or I would have appended them to this paper. The results obtained were encouraging, but, as the turpentine was sometimes uncertain in its action, I have not continued its use, except occasionally as a supplement to castor oil.

With regard to the topical application of substances to the ulcerated surface by means of injections, in only one of the cases quoted was this tried, the administration of the oil being stopped during the case. The injection, which consisted of bismuth, was given twice with the effect of at once increasing the number of stools, and adding greatly to the patient's distress. The enemata were at once discontinued, and the castor-oil treatment resumed, and the patient made a good recovery. In other cases where I have tried bismuth injection, and injections of other substances, the results have not been encouraging.—*Lancet, Feb.* 14 *and* 28, 1880, *p.* 241, 321.

31.—ON THE TREATMENT OF DYSENTERY BY RECTAL INJECTIONS.

By W. G. King, Esq., M.B., Surgeon I.M.D.

Dr. Houghton expresses a wish that the treatment of dysentery by bismuth injections, as recommended by him, should undergo a trial in a tropical country. I have pleasure in offering a confirmatory experience of that gentleman's estimation of this remedy under the desired circumstances.

In the course of large public practice in India, I have constantly had recourse to the exhibition of the subnitrate of bismuth, in cases of dysentery, both by the mouth and rectum. Its use by the mouth is by no means novel, the *rationale* of its action being the mechanical covering of the inflamed and ulcerated mucous membrane by a non-irritating and protecting film. Remembering that in a large proportion of cases the dysenteric mischief is situated in the lower part of the colon and rectum, I have always regarded an attempt to effect local treatment a matter of importance. In such cases benefit ensues as a result of directly curative measures; but even when an incorrect diagnosis of the chief site has been made—the actual position being higher up,—the effort to soothe the mucous membrane of the rectum, and allay spasm of the sphincter ani, amply repays the attempt by tending to the production of the *sine quâ non* of rest for the diseased bowel and the patients generally. The attainment of this object is, I think, frequently much assisted by the action of bismuth used per rectum. Although I have given this remedy as an injection, simply suspended in mucilage in the case of children (or in adults where opium in some form was being administered by the mouth), my usual plan has been to combine forty grains with half a drachm of tincture of opium, and, if intolerance of ipecacuanha by the mouth was encountered, also adding two scruples of that drug. In whichever form it was prescribed, I generally directed the bowel to be washed out in as gentle a manner as possible with lukewarm water, the object being to simulate the soothing process of fomentation; to procure, for contact with the drugs, a clean surface; and, finally, to diminish the chance of loss of injection by overcoming beforehand the spasm produced on the first introduction of the tube. Meddling with the rectum in this manner may seem opposed to the object of rest by unnecessary manipulation creating spasm, but I am convinced no injurious effect is caused. Far from this being the case, careful cleansing proves of the greatest utility by removing mucous and irritating secretions which, by their presence, excite tenesmus, a result analogous to the cessation of pain which may be witnessed after removal of a plug of tenacious mucus from the os uteri in cases of chronic metritis. Indeed, hydropathic (!) treatment of dysentery is by no means to be despised; I have seen a severe case of the acute form effectually checked by the use of the warm bath, fomentations to the abdomen, and copious warm water injections per rectum, without the use of any medicine. Two ounces of fluid, as used by Dr. Houghton, seems a somewhat smaller amount than usually desirable. As I regard the action of bismuth as merely

mechanical, I attempt to give it in larger proportion—four to five ounces—so that the protecting film may be well scattered over the rectal mucous surface. This amount, if the precaution of cleansing the rectum is attended to, is, as a rule, fairly well retained.

I may also mention I have found the soothing influence of a bismuth and opium injection beneficial after the employment of injections of nitrate of silver. In all cases, however, treatment by this method has been undertaken, as with Dr. Houghton, as merely subsidiary to the use of ipecacuanha by the mouth, if practicable. In the treatment of endless cases of famine diarrhœa and dysentery encountered during the late wretched famine in Southern India, the use of bismuth in ten-grain or scruple doses, combined with compound ipecacuanha powder, proved, after an honest trial of every feasible method, to be the most reliable treatment. Given at a time when it is calculated the intestine is fairly quit of food substances, this combination at once soothes the frail atrophied mucous membrane, and affords a film which protects its surface from the action of changed and irritating secretions.—*Lancet*, *Oct.* 18, 1879, *p.* 594.

32.—CONSTIPATION VIEWED AS A DISEASE "PER SE" AND AS AN EXCITING CAUSE OF DISEASE.

By Dr. ROBERT BELL, Physician to the Glasgow Institution for Diseases of Women and Children.

If we remember the power that the colon possesses of absorbing fluids (which numerous experiments place beyond a doubt), we do not require to ask an explanation as to the disappearance of the watery constituent of the stools in subjects suffering from constipation. I have heard it stated by a lecturer on the practice of medicine that "it was quite compatible with health to go a week, or even longer, without having an evacuation of the bowels"—that, in fact, it was all a matter of habit. Now, my observations go entirely to disprove such statements; moreover, such remarks are most apt to lead a student to look upon constipation as quite a trivial matter. If he does so, he will doubtless find that he has a great deal to unlearn, as he will soon discover that quite a host of complaints in every period of life may directly or indirectly take their origin in this abnormal condition of the bowels, and in very many instances the symptoms as a whole can be shown to be dependent upon this morbid condition. How is it that a smart purge, and a thorough clearing of the *primæ viæ*, have such a beneficial effect when one is feeling out of sorts? One rarely begins the treatment of any disease without being first satisfied as to the

state of the bowels and endeavouring to rectify them. A purgative will probably be given if there is constipation, and yet in numberless instances purgatives may be given over and over again without having the effect of clearing away scybalous masses from the lower bowel. When constipation has continued for a lengthened period, the colon becomes distended, and the sacculæ attain a greater capacity. The gut loses its tonicity, and consequently its power of contracting upon and expelling the fæces is reduced very materially. Thus we find that patients, when asked if they have a motion of the bowels every day, though they may insist that they are quite regular in that respect, yet, on close questioning, are led to confess that they never experience that sense of complete relief which they would wish to feel after a stool, but, on the contrary, that there is a desire to sit on the w.c. and endeavour to void more fæces. It will also be found that such individuals always complain of the fæces being lumpy and difficult to pass. Others again will inform you that, so far from their bowels being constipated, they have frequent attacks of diarrhœa, but it will not be difficult to discover that such attacks are invariably preceded and followed by constipation; moreover, that the diarrhœal evacuation contains hard pieces of feculent matter often described as pellets. How are these little attacks of diarrhœa to be accounted for? That it is not diarrhœa in the ordinary sense of the word is not difficult to show, as the stools are small in quantity, and do not come away with the rush that is generally a symptom of diarrhœa in its true type. This form of diarrhœa occurs most frequently early in the morning, often necessitating the patient rising out of bed and going to stool; indeed, there is no choice, as the desire is so urgent. There may be two or three such stools before breakfast, and the probability is there will be no more during the remainder of that day. Another characteristic of these stools is that they possess an extraordinarily offensive odour. It is quite evident that what we have to deal with at present is a "looseness" of only a limited portion of the intestine, and is due to the irritation produced by the presence of hard masses of scybala in the colon, which act as an irritant on that portion of the bowel and produce a catarrh in their immediate vicinity; the mucus which is excreted from the mucous membrane then acts as a partial solvent to a portion of the scybalous matter, at the same time a partial disintegration of it takes place; and so we find that the stools in this intermittent kind of diarrhœa always contain innumerable small pieces of hard fæces. In such attacks it must, of course, be worse than useless to employ the ordinary treatment of diarrhœa by opiates or astringents. The removal of the cause must be aimed at, and hence the importance of a correct diagnosis. It

will be generally admitted that when the fæces descend into the colon they are of a soft consistence; that they are *not* hard and dry. What has become then of their watery constituents when they are converted into hard scybalous masses? Sometimes so difficult to pass are they, that an evacuation of the bowels in such circumstances may actually produce more suffering than the pains of labour, and, as I saw only a few days ago, may really bring on premature parturition. I would repeat, what becomes of the watery portion of the fæces, and what is the effect on the general system, especially the nervous apparatus, of the hardened matter lodging in the bowel? We have not to look far for an answer to the first part of the question when we are cognisant of the power of absorbing liquids which the large intestine possesses. The fluid most certainly finds its way into blood, and thus of necessity produces a form of blood-poisoning. We may note the following effects produced by the absorption of such fetid matter,—the red corpuscles become diseased, they are altered in colour, diminished in numbers, have their carrying power lessened. Thus a sallow complexion results, dark rings appear below the eyes where the skin is thin and more transparent, the limbs and extremities are cold in consequence of oxygen being supplied in less quantity. There is a feeling of lethargy due to the blood being vitiated and the corpuscles being in this enfeebled state. Consequently the system is not nourished, and there is a falling off in flesh. Then there is a diseased blood supply circulating through the nervous system, and as a consequence nervous depression co-exists. The pulse becomes slow and easily compressed. There is a feeling of numbness in the extremities, the organs of digestion and assimilation are lowered in tone. There is loss of memory, and a want of power of concentrating the thoughts, a heavy drowsiness supervenes, which never seems to be removed by what appears to be really sound sleep, and all the functions of the body are carried on in an unsatisfactory way. We will find, too, that the patient is tired on very little exertion of any kind. Then there are numerous local symptoms produced by the presence of scybala in the colon, which very often are attributed to other causes. Among these may be noted most acute pain on pressure over the situation of the hardened matter, and so severe may this be that it may simulate peritonitis. If it occurs in women we often observe, as a consequence, ovarian neuralgia, this being excited by the irritation conveyed to the ovary by the presence of scybala in its immediate neighbourhood, while a predisposing cause exists in the lowered nervous tone consequent upon blood-poisoning. Frequently there is acute pain complained of over the ileo-cæcal valve, and in many, if not

the very great majority of instances, typhlitis is a direct consequence of the irritation produced by scybala lodging in the cul-de-sac. Irritability of the bladder is another local symptom frequently resulting from a similar condition of things in the immediate neighbourhood of the organ. Displacements of the uterus may be and are caused by large accumulations in the rectum, and in every case are very much aggravated by such a state of matters. I have seen cases where an accumulation of this kind has been mistaken for pregnancy, and at other times where it has been looked upon as an ovarian tumour. A very constant effect of this condition is the prevention of refreshing sleep at night. The patient always complains of dreaming, and nightmare is a frequent symptom of this torpid state of the bowels. It will invariably be ascertained that there is no refreshment from sleep, in fact the remark is generally made that he or she is more tired in the morning than they were on going to bed. In many cases I have found the temperature of the body rising at evening and falling again towards morning, so much so that typhoid fever has been suspected to be present. If children are the sufferers, a whole train of ailments may result, an idea of which will best be formed by a perusal of the report of cases given below.

In a most careful perusal of the most recent works on the practice of medicine, I cannot find in one of them the subject of constipation gone into *per se*, and even where it is mentioned as a concomitant of other diseases, it is barely glanced at. This is all the more strange when it is, beyond doubt, not only a disease having a most enormous train of painful symptoms, but is a predisposing cause of quite a host of other ailments.—*Lancet, Feb.* 14, 1880, *p.* 243.

33.—ON PHANTOM STRICTURE AND OTHER OBSCURE FORMS OF RECTAL DISEASE.

By Dr. W. H. Van Buren, Professor of Surgery in Bellevue Hospital Medical College, New York.

Muscular spasm may be considered as excluded in studying the causes of phantom or imaginary stricture of the rectum.

This singular affection will be found in most cases to take its origin in the constipation and difficulty of defecation so common in dyspeptic and hypochondriacal subjects; and the idea thus begotten of the existence of a physical obstacle in the lower bowel is too often confirmed by some professional opinion, based neither on careful physical exploration nor on full practical knowledge. The obstruction almost certain to be encountered when a bougie is inserted into the rectum, from contact with a fold of the gut or with the promontory of the

sacrum, is accepted as evidence of stricture; the novelty and simplicity and apparent effectiveness of the manœuvre please the patient; the impression produced upon the lower bowel provokes a fuller stool; and, when the bougie possibly enters more freely than usual, it is assumed that "the spasm is giving way." As an imaginary stricture is always located beyond the reach of the finger, a prompt demonstration of its non-existence is not easily arrived at; and the arrest of the bougie by the promontory of the sacrum has a singularly convincing effect upon the mind of the patient and of the physician. The invalid who once fairly enters on this vicious circle is therefore likely to travel in it indefinitely, for imaginary diseases are notoriously slow in getting well. The case recorded by Syme is worthy of repetition in this connection; it is authentic, bears directly on the point which I desire to make, and will always possess interest as a beacon against error. Like the legal cases admitted as precedents, it should be held in memory.

Case 1.—He introduces the case as "an elderly lady whom I saw with Dr. Begbie." She had been supposed to suffer from stricture of the rectum between five and six inches up the gut, and had been subjected to treatment for it, during several years, before coming under Dr. Begbie's care, "by two gentlemen of the highest respectability in this city. Finding that the coats of the rectum, though greatly dilated, were quite smooth and apparently sound in their texture, so far as my finger could reach, and conceiving that the symptoms of the case denoted a want of tone or proper action rather than mechanical obstruction of the bowels, I expressed a decided opinion that there was no stricture in existence. Not many months afterward the patient died, and, when the body was opened, not the slightest trace of contraction could be discovered in the rectum or any other part of the intestinal canal. One of the gentlemen who had been formerly in attendance was present at this examination, and, wishing to know what had occasioned the deception, which he said had led to more than *three hundred hours* being spent by himself and his colleague in endeavours to dilate the supposed stricture with bougies, he introduced one, as he had been wont to do, and found that upon arriving at the depth it used to reach its point rested on the promontory of the sacrum."

The following case, which occurred many years later in another hemisphere, is an example, apparently, of similar self-deception.

Case 2.—Mr. J. E., a dyspeptic middle-aged banker, from a western State, consulted me in 1854 in consequence of the drudgery to which he was subjected in being compelled to introduce a bougie into his rectum every other day for half an

hour "for a stricture"; and without this manœuvre he said he was unable to get a passage from his bowels. He had been pursuing this practice for nearly three years in consequence of advice received from more than one highly respectable professional source, and he was anxious for more effective and radical treatment. There was no stricture within reach of the finger, nor could I discover any evidences, rational or physical, of obstruction higher up, and, finding that his favourite bougie (for he had quite a collection of them) penetrated no further than my finger had reached, I suspected that the instrument was simply acting as a suppository in provoking action of the bowels, and that the real difficulty in defecation consisted in want of natural action arising from other causes. I encouraged the patient therefore to adopt a more rational and varied diet and a less sedentary life, and induced him, not without some difficulty, to discontinue the bougie. Mr. E. gradually became reassured, and adopted my opinion and suggestions. He subsequently took up his permanent residence in this city, and has since been uninterruptedly under my observation, but has never had any return of symptoms suggestive of stricture of the rectum.

But the promontory of the sacrum is not the only source of possible deception when the rectum is explored by a bougie beyond the reach of the finger. The walls of the rectal pouch tend to fall into loose folds when empty, and they present also certain slight permanent partial constrictions or narrowings. The uppermost one of these corresponds with the level at which the rectum gets its complete peritoneal investment. A normal narrowing at this point had been already frequently recognised, and the fact has been confirmed, by good observers, in the experiments in manual exploration recently practised upon the rectum.

Other writers have described slighter and more or less constant permanent narrowings at and below this point of the rectum as a "third sphincter," and, in fact, have cumbered the archives of surgery with a good deal of fruitless speculation concerning an organ to which anatomy and physiology have been equally unsuccessful in assigning either certainty of location or certainty of function. Dr. Chadwick, of Boston, has shown by his own and other recent researches, that all the so-called "internal sphincters" of the rectum, including the upper band of circular fibres spoken of as a "third sphincter," and differently described by Velpeau, Nélaton, and Hyrtl, are simply bundles of ordinary muscular fibres of the intestine, differing from each other only in volume and power, and that their function is *not to obstruct the gut and arrest its contents*, but, like all other circular intestinal fibres, *to urge by their peristaltic*

contractions the contents of the bowels onwards. He demonstrates that the true and only power of arrest resides in the external voluntary *sphincter ani,* aided by the perineal muscles; that the bundles of *involuntary* fibres, if entitled to any special designation, should be called "detrusors" and not "sphincters"; that they form a part of the apparatus by which peristalsis is effected, and that for this purpose they are under central control, being stimulated by the vagus, and inhibited by the splanchnics, dilating and contracting with a certain rhythmical precision under the reflected stimulus of fæcal distension.

For our present purpose Dr. Chadwick's researches concerning the "so-called internal sphincter" suggest two practical deductions: first, that the uppermost of these heavier bundles of involuntary fibres, especially when it forms a sort of upper limit to the rectal pouch posteriorly, often corresponds with a projecting fold of the rectum, and is liable to offer an obstacle to the passage of the bougie, and thus to simulate a stricture; second, that the nerve force which presides over the rhythmical contractility of the unstriped muscle of the intestinal walls is liable, under morbid influences, to diminish or to fail, and that atony of the *detrusor* muscular apparatus is not an uncommon cause of the difficulty in defecation which is ascribed to stricture. I would remark also, by way of enforcing the importance of the latter deduction, that the evolution of gases and their absorption by the intestinal surface are phenomena admitted to be due to disturbed—generally defective—nerve power; so, also, is the diminished follicular secretion of lubricating mucus, causing unnatural dryness of these surfaces; and that these conditions, which often accompany, if they do not actually cause, failure in function on the part of the large bowel, are remedial in most cases by rational therapeutics.

Case 3.—General D., æt. 47, a fine-looking man, of good constitution, married, habitually overworked in railroad management in the West, came to the city in March 1877, in search of relief from a difficulty in defecation, from which he had suffered for ten years; speaking of his malady as a "stricture of the rectum," which, he had been told, existed at five inches from the anus. In a note of his case taken by my associate, Dr. Keyes, the rectal trouble is referred to as "chronic diarrhœa and dysentery with ulceration of the lower bowel during service in the army from 1861 to 1865, the symptoms gradually assuming an obstructive character." The stools were scanty and difficult, "pipe-stem" in size, never secured without a previous enema; "chunks of fæces" and "wads of pus" are spoken of as sometimes expelled. The diagnosis of stricture made, according to the patient, in Chicago, Ill., in 1868, was afterwards confirmed by Dr. Van Buren in N.Y., who advised a trip

abroad; by Nélaton and Dolbeau who were consulted in Paris, and afterwards by Hamilton, of Dublin. All advised continuance in the use of bougies, declining to operate so high up. In despair of recovery he now revisits New York, determined to have an operation performed for his relief.

The patient was accordingly etherised for thorough exploration, and, after stretching the sphincter, a rectal caoutchouc bougie of the largest size was passed ten inches into the bowel without difficulty. On withdrawing the bougie after largely inflating its terminal bulb, the latter was slightly obstructed by what seemed to be a muscular band at about five inches. As far as could be ascertained by inspection, this band was not cicatricial, there was no evidence of the previous existence of any ulcer at the spot, and the mucous membrane above it was healthy. To remove all doubt I felt justified in requesting Dr. Keyes to introduce his hand, which measured nine inches around, into the rectum. It passed readily through the supposed stricture, and into the sigmoid flexure. The muscular band was recognised, but yielded with the slightest resistance. It was situated at the upper limit of the rectal pouch, about on a line with the reflexion of the peritoneum. The patient was somewhat sore for a day or two, but suffered no serious harm from the exploration.

I was struck by one of Gen. D.'s reminiscences, which occurred to him in the effort, apparently, to reconcile himself to the conclusion which I was enforcing that he certainly had no stricture. "Well," said he, "I remember thinking how strange it was that when I was camping out during the grouse-shooting season I could always go out in the bush and get a good satisfactory stool every morning."

As he had derived positive benefit for the time from his former trip abroad, I advised him to cross the ocean again, to use a laxative pill containing the choleate of soda, and, when he returned to business, to interpolate as much grouse-shooting as possible.

I am indebted to Dr. Keyes for the record of the following case:—

Case 4.—Rev. I. B., 41, unmarried, applied for relief in June, 1877, for stricture of the rectum high up. Thinks he has two strictures. Has been suffering from dyspepsia and constipation for fifteen years. Has used much medicine, and, by the advice of an irregular practitioner of much popular reputation in rectal diseases, has been introducing a large bougie six inches or more up the rectum four times a week, at intervals, for several years. Says, moreover, that his "stools are uniformly flattened," and that he voids an excess of rectal mucus.

No evidences of stricture could be found on the most thorough exploration.

This patient had suddenly taken to hard study in order to become a clergyman, after years of active and laborious life in another capacity in the open air; he has a marked gouty constitution, and inherits dyspepsia. He was disabused as to the existence of any strictures, advised more outdoor life and a laxative pill, as in the last case. Some time afterwards a grateful letter was received from Mr. B. announcing a decided change for the better.

The terms "pipe-stem," "flattened," and "tape-like," as applied to the appearance of stools, and also the expression "wads of pus," suggest two common sources of error in diagnosis; for these appearances do not necessarily mean stricture, nor even ulceration looking to subsequent stricture; and as in the cases just detailed they may be entirely delusive. In true stricture the fæces, when solid, are in small lumps, resembling sheep-dung. An elongated mass must necessarily take its shape and size from the orifice of the anus, through which it is last extruded. The only exception to this is when the sufferer, by extreme straining, has forced his stricture far enough through the anal opening to get its own final imprint upon the extruded material; and this I have known to occur. Then, again, it is a very common occurrence for a victim of fancied rectal disease to report the daily evacuation of pus, which, on inspection, is found to be simply the normal jelly-like rectal mucus, which is always stained more or less deeply yellow by the bile in the fæces. This mucus, especially where efforts at stool are frequent, is always more liberally secreted, and its quantity always greater, than is generally supposed. Moreover, this pus-like material is often streaked with blood, or accompanied by positive hemorrhages from hemorrhoidal congestion aggravated by straining. These appearances are very likely to suggest the presence of serious disease to any anxious patient who watches himself too closely.

Quain, who says he has "known repeated instances in which the bougie has been passed at short intervals for supposed or imputed stricture, where none had ever existed," quotes the following case:—

"A medical practitioner under Dr. Burne's care for broken health from excess and irregular habits, was persuaded by a surgeon that his ailments were due to stricture of the rectum, although his bowels acted regularly every day, and, contrary to the advice of his physician, he submitted to treatment with a bougie, but, after some months' trial, became convinced of its uselessness, and gave it up. On dissection of the body, which was made not long after, 'no stricture nor trace of stricture could be discovered, either in the rectum, colon, or intestinal canal.'"

Finally, medical men, even where their own sensations are in question, are liable to deceive themselves. Obviously there is no lack of evidence that honest men may fall into error as to the existence of stricture of the rectum. Many cases like those first recorded have doubtless never seen the light. Moreover, patients often manifest a singular facility, with which practical men are familiar, for being persuaded of the actual existence of disease which is entirely imaginary; and, unhappily, there are amongst those to whom patients appeal, men who do not scruple to take advantage of their credulity. But there are also, I am convinced, many well-read practitioners who have been led into error by the common but vaguely expressed belief in "spasmodic stricture" of the rectum, implied rather than openly defended, by many good English authors. As I have, perhaps, passed over this point in too summary a manner, I will discuss more fully the considerations which have led me to believe in the non-existence of obstruction in the rectum from this cause; for the idea of spasm as a cause of stricture of the rectum has secured such a degree of favour in the professional as well as in the popular mind—as in the case of the urethra—that the simple assertion that it is not admitted by modern authorities, may not be accepted as final.

The fanciful theory that spasm of the voluntary muscle surrounding the membranous urethra comes by reflex action from organic stricture formed nearer to its meatus, which has been made the basis of so much doubtful practice in this country, is, happily, for anatomical reasons, not applicable to the rectum. But in the earlier half of this century a similar delusion as to the great frequency of rectal stricture, its origin in spasm, and its curability by the bougie, was so prevalent in England that the fear of this disease grew to be a national peculiarity. It was, in fact, an example of the "epidemic terror" which Goldsmith had described in the Citizen of the World as one of the peculiarities of his countrymen, and which was prevailing at the time he wrote in the form of a dread of mad dogs.

A recent French author repeats an illustrative story of a lady in London who, recommended to a specialist for costiveness, was examined with a bougie and pronounced strictured. Her husband, surprised and angry at the liberty taken with his wife, rushed off with a horse-whip to the house of the offender, but came home again after a little to his anxious partner confessing that he had grievously wronged a most worthy gentleman. The specialist had not only satisfied him as to the certainty of the lady's malady, but had proved, by inserting a bougie, that he also had a stricture. Horace Walpole furnishes similar evidence in one of his letters. He warns a friend, who

is about going to Bath for his health, not to fall into the hands of a notorious practitioner of that place who always found his patients affected with contraction of the lower bowel and set them to introducing bougies. His friend in response warns Walpole not to joke about serious matters, for he had already consulted Mr. ——, who had actually found an obstruction in his bowel that caused all his symptoms, and that he was already getting better under Mr. ——'s skilful use of an instrument which he was inserting daily.

Marshall Hall in one of his published lectures relates the case of a young medical friend who had been nine weeks under his roof, and in whom he had never suspected any disease of the bowel, who went to Bath and met there a young surgeon who passed a bougie and fancied he discovered stricture of the rectum. The bougie was inserted daily for several weeks, causing much pain and irritation. Dr. Hall subsequently took his friend to Sir Charles Clark, an eminent surgeon of that day, who examined him and found no stricture. The bougie was relinquished, and from that time "the factitious disease of the rectum, for such it was, was forgotten."

It is a remarkable fact that there is little if any evidence concerning either spasmodic or fancied stricture of the rectum to be found in the surgical literature of the Continent. The Germans and French scarcely allude to spasm, except as a complication of organic narrowing, and then with no emphasis or precision; and by the latest French authorities, Mollière and Trélat, the existence of pure spasm as a cause of rectal stricture, except where the gut is in the grasp of the external sphincter, is formally denied.

But by English authors the subject is handled with less decision. One of the most recent and best of them, Mr. Allingham, bases an opinion as to the frequent presence of spasm, *as complicating organic stricture*, upon the most positive clinical experience I have found recorded anywhere.

"There are, no doubt," says he, "many cases of stricture in which there is very little deposit and much spasm; and there are, on the other hand, cases where much obstruction exist but very little spasm. A patient under my care at St. Mark's had a stricture so tight that I could not make the point of my little finger enter it; on putting her under the full influence of chloroform I could get two fingers through without difficulty."

Admitting, not without some hesitation, Mr. A.'s first allegation, which is possibly somewhat influenced by English traditions, I am forced to confess that I have never felt any rectal stricture like that of this woman, and should suspect the presence of hysteria in this case, for it would seem that an excep-

tion to the rule ignoring pure spasmodic stricture must be made in favour of this extraordinary disease, which so often defies all our rules. I have seen continuous spasm of the *voluntary* muscles of the leg and foot simulating well marked talipes varus in a hysterical young woman, which disappeared under the influence of chloroform, but always returned. She ultimately submitted to a division of the tendo Achillis in one of our hospitals, on which the local malady returned no more. Are we justified in assuming that similar continuous spasm of *involuntary* muscle may exist? Jaccoud records a case of fæcal vomiting which occurred in his wards at the Lariboisière, in 1867, in a young woman who was admitted with hysterical convulsions. For eight days this person, after the convulsions ceased, at least once, sometimes twice, in the twenty-four hours, vomited veritable fæces, dense, solid, cylindrical, of a brown colour, and with the normal fæcal odour, coming evidently from the large intestine. Jaccoud witnessed the act himself, and so also did Dieulafoy, his *interne*, and he characterises it as actual defecation by the mouth. Apart from the passing disgust which followed the act, the patient ate as usual, and continued in her ordinary health, except in the absence of normal action of the bowels. At the end of the eighth day the hysterical convulsions returned, and, coincidently, the fæces resumed their natural route. All possibility of deception seems to have been rigorously excluded. Within a fortnight this woman was seized with grave typhoid fever, and died. Careful examination of the body disclosed no mechanical obstruction whatever in the intestinal canal. The ileo-cæcal valve was normal.—*American Journal of the Medical Sciences, Oct.* 1879, *p.* 317.

34.—ON THE PHYSIOLOGICAL ACTIONS OF DRUGS ON THE SECRETION OF BILE.

By Dr. WILLIAM RUTHERFORD, F.R.S., Professor of the Institutes of Medicine in the University of Edinburgh.

As the liver is an organ whose due activity is indispensable for the maintenance of health; as it is frequently the subject of disorder, and consequently receives a large share of attention from the physician, it is obviously important that he should possess precise knowledge of the manner in which it is affected by medicinal agents.

Croton Oil.—As Röhrig placed croton oil at the head of his list of hepatic stimulants, we made this the subject of three experiments which convinced us that this substance so slightly stimulates the liver that its effects thereon are unworthy of attention.

Magnesium Sulphate is another substance which, according to Röhrig, is a stimulant of the liver, though a feeble one. Two experiments with this substance convinced us that this is an error. So far from increasing, it *diminishes* the secretion of bile.

Sulphate of Manganese is stated by Ure to be a cholagogue as well as purgative when given to man in doses of from 60 to 120 grains. Dr. R. H. Goolden states that in doses of from 10 to 20 grains it produces large bilious evacuations. Two experiments proved that in the dog sulphate of manganese stimulates the intestine but not the liver.

Castor Oil was found by Röhrig to have little effect on the bile-secretion, but two experiments with doses which produced purgation proved that the secretion of bile is diminished when the purgative effect becomes fully established.

Ammonium Chloride.—According to Garrod's Materia Medica, chloride of ammonium is "by some considered a cholagogue." Two experiments with this substance enable us to say that in the dog it does not stimulate the liver. In the case of a small dog we gave eighteen grains in three successive doses of six grains, and decided hydrocatharsis ensued. The only result was diminished secretion of bile. In the case of a large dog, 130 grains were given, in successive doses of ten, twenty, forty, and sixty grains. The purgative effect was moderate. The bile secretion was at first unaffected, but afterwards fell.

Gamboge is by no one said to be a cholagogue, still we used it in two of our earlier experiments for the purpose of ascertaining how the liver would be affected by a substance which powerfully irritates the duodenal mucous membrane. In one experiment small doses of gamboge were repeatedly injected, and although the irritation of the mucous membrane of the duodenum and remainder of the small intestine was very marked, the bile-secretion simply fell as the hydrocatharsis set in. This and several other experiments proved that duodenal irritation is *not of necessity followed by excitement of the bile-secreting mechanism.*

Hepatic Depression from Intestinal Stimulation.—The results of our experiments with sulphate of magnesia, castor oil, chloride of ammonium, and gamboge show that when a substance stimulates the intestinal glands, but not the liver, hepatic action is depressed, and the production of bile is lessened. We invariably observed that, while slight purgation—by a purely intestinal irritant—scarcely, if at all, depressed the secretion of bile; powerful purgation produced a very marked effect.

Resina Podophylli is a very powerful hepatic as well as intestinal stimulant. Its effect on the intestinal mucosa is so irritating, that it seems feasible to regard it as contra-indicated in cases where there is a tendency to irritation of that mem-

brane. If the dose be too large, and violent purgative action ensues, the secretion of bile, so far from being increased, is diminished. With a smaller but still too powerful purgative dose, the bile-secretion, though it may be powerfully raised for a short time, quickly falls as the substance passes down the intestine and induces secretion from a greater and greater number of Lieberkühn's glands. With somewhat smaller doses the increased bile-secretion is much more prolonged, although the hepatic excitement is not so intense at the outset as in the preceding case. It is manifestly of great importance to keep these facts in view in practical medicine.

It seemed to me desirable to test the action on the liver of some substances used by the American "Eclectics," and I selected the following: euonymin, iridin, leptandrin, sanguinarin, baptisin, phytolaccin, hydrastin, juglandin. These substances are prepared by Keith and Co., and by Tilden and Co., of New York. They are not active principles but only impure resinous matters. Messrs. Keith have informed me that the crude roots of the plants are dried, crushed, and percolated with alcohol. The alcohol is then evaporated or distilled off, leaving the active principles in the form of an extract, which is then "freed from impurities," dried, and pulverised. How it is *freed from impurities* is not stated.

Resina Iridis and Resina Euonymi, or "Iridin" and "Euonymin."—These two substances are prepared respectively from the roots of the *Iris versicolor* and *Euonymus atropurpureus*. Both powerfully stimulate the liver, while they do not powerfully stimulate the intestine of the dog. Although not so powerful as podophyllin, they will both doubtless be preferred in many cases to that substance, because of their far milder excitement of the intestine. For not only is the latter in most cases advantageous on its own account, but also because the action on the liver is far less liable to be hampered and diminished by the intestinal stimulation. This, as we have seen, is apt to be the case with podophyllin. Our experiments on the dog led us, and have led many others, to try the effects of these substances on man, and they are of great value. The average dose of iridin is four grains; of euonymin two grains. In either case two grains of extract of hyoscyamus should be added, and taken at bedtime, for without this some persons experience griping. Neither substance produces headache or any sickness. In some persons the above doses of both substances produce a sufficient purgative effect; but in other cases the purgation is insufficient or delayed, and griping is then apt to ensue. The most beneficial result is obtained by following the dose of these remedies by a mild saline aperient, such as Püllna or Carlsbad water, on the following morning, so that

the bile secreted during the night may be fully and quickly removed. I have in my own case noticed slight depression after four grains of iridin, which I never observed after two grains of euonymin. I therefore inferred that the latter is preferable when repeated stimulation of the liver is desirable. It is however important to remember that although euonymin usually suffices to quickly remove a slight feeling of biliousness; iridin is, I am convinced, the more powerful remedy of the two, when the tongue is decidedly yellow. I have in such a case been more than once surprised to find that on awaking in the morning after taking four grains of iridin the previous night the yellow tongue and the bilious sensations were entirely gone. Since the publication of our results these remedies have come into very general use. Mr. Hardyman, of Cardiff, states that he has used euonymin in two-grain doses at bedtime in over fifty cases of biliary derangements and sick headache, and finds it of much value. Finding that in most cases one dose is insufficient he gives two grains at bedtime on two successive nights, following it each morning with a saline purge.

Resina Hydrastis, or "Hydrastin."—The root of the *Hydrastis canadensis* is admitted by all to be tonic, and by some it is said to be aperient, cholagogue, diuretic, antiseptic, &c. "It has been employed in dyspepsia, and other affections requiring tonic treatment, in jaundice and other functional disorders of the liver, as a laxative in constipation and hemorrhoids, and as an alterative in various diseases of the mucous membranes, such as catarrh, chronic enteritis, &c. By some it is used as one of the best substitutes for quinia in intermittents." These and other statements regarding it are made by Wood and Bache, who further aver that a "more precise investigation of its physiological and therapeutic properties is necessary before we can venture to decide its place among medicines." It contains an alkaloid, hydrastia or hydrastin, which has been found to be identical with berberin. The "hydrastin" employed in our experiments was not the alkaloid, but a resinous substance prepared from the root of the plant by Keith and Co. The dose for a man of this preparation is from one to two grains. Our experiments prove it to be a moderately powerful stimulant of the liver and a feeble stimulant of the intestine. Considering also its tonic properties, it seems to be a substance eminently worthy of the attention of the practitioner.

Resina Juglandis, or "Juglandin," is an impure resin prepared as above from the root of the butter-nut (*Juglans cinerea*). Regarding the properties of the bark of the butter-nut, Wood and Bache state that it is a mild cathartic, operating without pain or irritation, and resembling rhubarb in the property of evacuating without debilitating the alimentary canal. It was

much employed during the late American civil war by Dr. Rush and other army physicians. It is especially useful in habitual costiveness and dysentery. Nothing is stated regarding any influence on the liver. An extract of the bark is officinal in the United States. The dose of Keith's juglandin—the substance used in our experiments—is from two to five grains. In the dog it is a moderately powerful hepatic and a mild intestinal stimulant. It seems to be worthy of the practitioner's attention.

Resina Baptisiæ, or "Baptisin," is prepared as above from the root of the wild indigo (*Baptisia tinctoria*). The root of this plant is said to be a powerful emetic and cathartic in large, and a mild laxative in small, doses. Stevens of Pennsylvania recommends a decoction of the root in epidemic dysentery. It is said to have proved useful in scarlatina, typhus fever, and in that state of the system that attends mortification. The physiological actions of this plant have apparently not been investigated, and it is nowhere stated that it is a cholagogue. The dose of baptisin for a man is from one to five grains. Our experiments prove this substance to be, in the dog, a hepatic and also an intestinal stimulant of moderate power. It may possibly be found of service as a hepatic stimulant in cases of torpid liver with a depressed condition of the system tending to gangrene. We commend it to the attention of the physician.—*Practitioner*, *Nov.* 1879, *p.* 321.

DISEASES OF THE URINARY ORGANS.

35.—ON LATENT ALBUMINURIA: ITS ETIOLOGY AND PATHOLOGY.

By Dr. GEORGE JOHNSON, F.R.S., Professor of Clinical Medicine; Senior Physician to King's College Hospital.

It is a matter of common and in fact every-day observation, that the urine of persons apparently in perfect health is often found to contain more or less albumen. The discovery of what had been a latent and unsuspected albuminuria is often made by accident. Within the last twenty years, at least half a dozen members of our profession have come to me with essentially the same statement, which was to this effect: "I was testing some albuminous urine, when it occurred to me to compare the action of the tests upon a presumed healthy specimen. I therefore tested my own urine; and, to my dismay, I found it highly albuminous." During the last few months, I have been consulted by three men, each of whom, believing himself to be in good health, and having proposed an insurance on his life, had been found to have albuminuria by the medical adviser of

the insurance-office. Quite recently, an apparently healthy young man told me that, seeing his medical brother testing some albuminous urine, he said to him jokingly, "You may as well test mine"; and the result was that it was found to contain an abundance of albumen. About the same time, a surgeon called on me and said that he had been made very anxious by the accidental discovery that one of his children had albuminuria. The eldest son, who was studying practical chemistry, had busied himself in testing the urine of every inmate of the house, when he discovered that a younger brother, apparently in good health and attending a day-school, had albuminuria.

These are a few instances out of a large number that have come under my observation in which the existence of albuminuria, heretofore latent and unsuspected, has been discovered as it were by accident.

The attention of the profession has recently been especially directed to the subject of what I propose to call latent albuminuria by several physicians, especially by Dr. Moxon (Guy's Hospital Reports, 3rd series, vol. xxviii), by Dr. Clement Dukes (British Medical Journal, Nov. 30, 1878) and by Dr. Saundby (British Medical Journal, May 10, 1879; also Birmingham Medical Review, July and October 1879). My main object in this communication is to maintain, first, that this latent albuminuria—albuminuria, that is, unassociated with any other evidence of functional disorder or structural disease—may, by a careful inquiry, be traced back, in a very large proportion of cases, to some probable exciting cause; secondly, that the presence of even the smallest trace of albumen in the urine is always pathological, never physiological, as I have somewhere seen it suggested that it may be; and that the neglect of this indication of a pathological condition and tendency, especially such negligence as involves repeated exposure to the exciting cause, may convert a temporary and occasional into a persistent albuminuria, which sooner or later, though it may be after many years, will result in a fatal disorganisation of the kidney.

In prosecuting the important practical inquiry into the probable cause of a recently discovered albuminuria, we shall not unfrequently come upon a history more or less clear and definite of an acute nephritis, either with or without dropsy, dating back for a period varying from a few months to several years. The acute renal disease may have been the result of exposure to cold and wet, or it may have occurred in connection with one or other of the following diseases: scarlet fever, measles, diphtheria, erysipelas, typhus or typhoid, pyæmia, rheumatic fever, etc.; or it may have been associated with the puerperal

state, either before or after parturition. The acute symptoms having passed away, convalescence has appeared to be established; but, in some cases, the patient having been kept under careful observation, the albuminuria has been found to continue for months and years. In other cases, there has been a less careful and prolonged observation, and the history, therefore, is less complete. Convalescence having been apparently established, no further examination of the urine had been made until after an interval of months or years, when albuminuria is found to be present, and there is great reason to believe that it has continued from the time of the previous illness. The practical lessons to be deduced from facts such as these, which are matters of every-day observation, are. 1. To test the urine for albumen repeatedly during the progress of all febrile and inflammatory diseases until convalescence has been completely established; 2. When, in the circumstances referred to, albuminuria has been found to exist, to keep the patient under observation, and to test the urine until it has been found continuously free from albumen; care being taken to test for albumen not only after rest in bed and before breakfast, but after food and exercise. As a general rule, it will be found that, whatever may be the cause of the albuminuria, the albumen is twice as abundant after food and exercise as before breakfast; and I have met with a considerable number of cases in which the urine, before breakfast having been free from albumen, is found to be more or less copiously albuminous after food has been taken. In all cases, therefore, of actual or suspected albuminuria, I ask to be supplied with two specimens of urine: one passed on rising in the morning, the other secreted two or three hours after a meal. In some exceptional cases, food has less influence than exercise on the production of albuminuria. A distinguished London physician, whose successful career was cut short by degeneration of the kidneys, found that, at the onset of his malady, albumen appeared only occasionally after walking exercise, when it was present in large amount. Food at the commencement appeared to have no influence on the production of albuminuria; but after a time there was persistent albuminuria, and ultimately death from uræmia.—*British Medical Journal*, *Dec.* 13, 1879, *p.* 928.

36.—ON THE THERAPEUTIC EFFECTS OF ALKALIES IN DIABETES.

Dr. Cornillon has studied at Vichy the effects of the waters upon diabetic patients. He finds that after the fourth or fifth day, sometimes even sooner, the thirst and dryness of the mouth become less troublesome, and inappreciable after the

tenth day in favourable cases, and after the thirtieth day in the most obstinate. The patient passes better nights, and the sleep is calm. During this period the urine, previously acid and light coloured, has become alkaline and of a yellowish orange. The appetite, instead of being disordered, becomes regular, the improvement being most marked at the end of the first week, and it may be said broadly that the alkaline treatment tends to re-establish the functions of the stomach and intestines. The patients, who have previously lost much flesh, begin again to make up and even increase upon their original weight. The dryness of the skin is replaced by suppleness, whilst the sweat reappears. The vinegar-like smell and the anaphrodisia remain, however, with the most enduring obstinancy, but the latter symptom yields to a certain extent after the continuance of the treatment for some years. As the diabetes diminishes the dimness of vision tends also to decrease. If hemorrhage has occurred in the eye, however, no treatment with alkalies will restore the impaired visual faculties. The alkaline bath is, according to Dr. Cornillon's experience, the best method of applying the treatment locally, and he does not hesitate to order it every time it is felt to be needed, in concurrence with internal treatment. In tuberculosis occurring in diabetic patients, Dr. Cornillon believes that the use of alkalies is not contraindicated. He finds that the pulmonary lesions are not extended, whilst the general symptoms produced by the tubercle are slightly diminished, the symptoms produced by the diabetes, such as thirst and weakness, the daily loss of sugar, &c., disappearing or decreasing under the alkaline treatment. Dr. Cornillon terminates his article in the following words:—It sometimes happens that in diabetes uncomplicated by progressive anæmia the treatment with alkalies gives no results; but this want of success is not common, and when it occurs it is to be attributed rather to an idiosyncrasy of the patient than to failure of the remedy. It may occasionally be noticed that the favourable influence of the alkalies is only transient, that the thirst and dryness of the mouth soon return, and that the sugar reappears in the urine if it had disappeared, and increases in quantity if it had previously undergone a diminution; it does not, however, attain to its original amount.—*Le Progrès Médical; Practitioner, March* 1880, *p.* 207.

SURGERY.

FRACTURES, DISLOCATIONS, AND DISEASES OF THE BONES, JOINTS, ETC.

37.—ANTISEPTIC TREATMENT OF LARGE OPEN WOUND.

By JOSEPH LISTER, Esq., F.R.S., Professor of Surgery in King's College, London.

There is a case which I wish to dress before you to-day—that of the woman from whose back I removed a large recurrent fibroid tumour twelve days ago. This tumour had first shown itself fifteen years since. It had been removed and recurred, removed again and recurred at intervals of three or four years, the woman each time foolishly allowing it to grow to large size before she sought the surgeon's aid. On the occasion of its fourth removal it was found adherent to the spinous processes of some of the dorsal vertebræ, which were accordingly scraped by the surgeon who performed the operation. When the patient again applied to him, with a large tumour in the same situation, he recommended her to my care. Finding the tumour of firm consistence, well defined, and freely movable, except about its middle, where it lay over the spine, and considering its very chronic course, I thought it right to give the poor woman another chance, and accordingly I removed the large mass taking with it the skin that lay over the growth, about five inches in breadth, and all the scar of the previous operations, the incision being about a foot in length. Finding the tumour firmly adherent to the spinous processes of four of the dorsal vertebræ, I removed these processes in almost their entire length with cutting pliers, and also a considerable mass of spinal muscles in their vicinity, leaving a large hollow in the middle of the back. I was desirous that this deficiency should be closed in immediately if possible, and accordingly, by a plastic operation, I brought a large flap of skin inwards from the right side of the back, to cover it.

In adjusting the flap in its new position, I found great advantage in the use of button-stitches of relaxation. I have for some time past used the lead buttons with a modification of form which makes them better adapted for their purpose.

[We have not room to give minute details in the operation on

this case, but the rest of the lecture, especially on what Mr. Lister calls his *protective*, is interesting. He says:]

I now apply the dressing. The protective, dipped in the 1 to 40 watery solution to destroy the septic property of any dust adhering to it, is applied over the healing wound. Immediately over this is placed a piece of the carbolic gauze, dipped also in the lotion, to ensure that it is actively antiseptic when applied, for the dry gauze cannot be trusted in this respect, and over this the regular gauze dressing of ample dimensions. Remember the immense importance of having the protective overlapped well on all sides by the gauze. The protective, having nothing antiseptic in its composition, must be overlapped just as if it were itself a wound.

The bandage is so arranged as to brace the arms somewhat back, though that is not now so necessary as it was in the first instance. Here again, from the liability of the chest to movement, we use the elastic bandage—one piece round the shoulders and another round the waist—to keep the edges of the dressing constantly applied to the skin.

The attainment of the results which you have seen in this case depends not only on the efficacy of the antiseptic measures used, but also upon the use of an efficient protective, as we term it—that is to say, a layer of material employed to protect the healing part from the irritation of the antiseptic itself. And with reference to this point, I may mention that we have now a considerably better protective than we had some time ago. It still consists essentially of oiled-silk, varnished on both sides with copal varnish. The copal varnish is the principal agent in this protective for keeping the carbolic acid out; it is much less permeable to carbolic acid than oiled silk is, although oiled silk itself is much less permeable to the acid than gutta-percha or caoutchouc is. The copal varnish, then, is the most important of the ingredients of which this protective is composed. Some time ago one of the manufacturers who supplied this protective, for reasons best known to himself, omitted the use of the copal varnish altogether, and supplied us with what was evidently mere oiled silk varnished with dextrine (which is put on outside the copal to ensure that the material shall take an antiseptic film when dipped in carbolic lotion). We were supplied then with mere oiled silk brushed over with dextrine, and this is a comparatively ineffective protective. On the other hand, that which we now use is coated on both sides with specially thick copal varnish, and this makes it correspondingly effective. I understand that some persons object to this protective as being too thick and substantial. These gentlemen little know what a mistake they make in thus objecting to it. When it comes to be of the tem-

perature of the body, it becomes perfectly soft and pliable; it applies itself beautifully to the part, so that there is no objection to it on that score. If we had not used a pretty effectual protective, we should not have had the kind of results you have seen in this case. If we had had a carbolic-acid dressing acting directly on the tissues of that exposed wound we should have had before now granulation and suppuration—suppuration occasioned by the irritation of the antiseptic—"antiseptic suppuration," as I have termed it; and if we had had the gauze dressing directly applied to that piece of dead tissue, it would have been impregnated with the pungent antiseptic, and would have become chemically stimulating on the same principle, though in a less degree, as if it had been putrid; it would have stimulated its living neighbours and urged them to granulation and suppuration, and we should have had a piece of dead tissue already separated by antiseptic suppuration.

Again, if we had not used a pretty efficient protective, we should not have seen the blood-clot organised and cicatrising without suppuration at the edge of the window in the skin. We should have seen it with a granulating and suppurating margin. I am induced to speak on this point because I believe the protective is often as yet imperfectly understood. A very eminent surgeon was round my wards some months ago, and he told me he had never seen the organising blood-clot. I took him to two cases in which it was occurring. He was a man of scientific precision, and he at once said, "These cases are enough for me; I know from this time that an exposed blood-clot may become organised." How was it he had never seen it, for he had practised antiseptic treatment with great efficiency for years? The truth was, as he told me, he had not thought the carbolic acid particularly irritating, and he had never used any protective at all, and therefore had never seen an organising blood-clot. I had sent to me some time ago by a very distinguished Russian surgeon this beautiful-looking article—this transparent film,—which he told me he now always used in preference to our protective—a most lovely thing, certainly enough to charm us to use it by its mere beauty. But I found it would not stand the test which will enable you always to ascertain the quality of a protective. I took a piece of it, and put upon the middle of it a little bit of lint soaked with 1 to 20 carbolic-acid lotion, and left it awhile; in the course of about half an hour, on applying the tip of my tongue to the other side, I perceived the pungent sensation caused by carbolic acid, showing that it had already gone through it; whereas, with a piece of this properly made protective, treated in the same way, you would find, after the lapse of three or four hours, absolutely no trace of the acid. Therefore, although this so-called protective is

very pretty, it is scarcely a protective at all in the sense in which we use the term. It will keep the wound moist, and thus prevent the dressing from striking to it, but that is nearly all it will do.

But worse remains to be told of this elegant article. The chemist who sent me the samples informed me that it contained thymol and salicylic acid in its composition; in other words, it was not only inefficient as a protective against the antiseptic, but it was actively antiseptic in its own substance. And he added, it could be made up with carbolic acid; showing that he had no idea whatever of the object for which we use the protective. It has been said sometimes that our dressings irritate wounds. The wound you have just seen is an instance to the contrary. It is not irritated; it is healing kindly, and not only so, but it shows very much less marks of irritation than you could get with any ordinary treatment that has been devised. But if surgeons use the antiseptic agent, whatever it be, in such a way as to act directly on the tissues, they will necessarily have their wounds more or less irritated according to the nature of the antiseptic they employ.

But, useful as a protective is in promoting the best kind of healing under an antiseptic dressing, I desire to say, by way of warning, that it would be infinitely better to use no protective at all than to employ it amiss; for, though at the risk of seeming tedious, I must repeat that in proportion to the efficiency of the protective in excluding the irritation of the antiseptic, is its power to exclude also its antiseptic virtue, and thus to conduct putrefaction inwards to the wound, unless it be overlapped well on every side by the really antiseptic element of the dressing.—*Lancet, Dec.* 20, 1879, *p.* 903.

38.—ON ANTISEPTIC SURGERY.

By William Mac Cormac, Esq., F.R.C.S., M.A., Surgeon to St. Thomas's Hospital.

I suppose that it may be assumed that all surgeons, in some form or other, employ antiseptics; and I think their general employment, which, during the last fifteen years, has become more and more apparent, is very largely due to the attention drawn to the subject by the particular method of employing antiseptics with which the name of Professor Lister is associated. It would be impossible within the limits of a short paper to discuss the different ways in which antiseptics are used in the treatment of wounds. I therefore propose to confine myself to Lister's particular plan of employing antiseptics, including any modifications which act on the same theory and

principles. Other methods I would term inexact; the Listerian method is, in contradistinction, an exact method, founded on a special theory, and carried out in all its details in almost precisely the same manner in each case. The theory is simple. Pasteur's beautiful experiments, which, so far as I know, have never been controverted, prove that it is not the air, but something contained in it, which excites decomposition. The active agent concerned is believed to be certain minute vegetable organisms, and their germs, which seem to be all present, which exist in many varieties, and probably act in many different ways, but which are capable of being developed *ad infinitum* wherever they discover suitable nidus. Whether micrococci and bacteria are the essence of disease, or mere agents in producing changes in the animal fluids, which become capable when thus changed of producing poisonous influences upon the organism, need not now be discussed. One great fact is certainly known—that the access of ordinary air to a wound is very generally the cause of inflammatory or putrefactive changes in that wound; while experiment has proved, and clinical experience corroborates the experiment, that air which has been filtered, or overheated, or subjected to the effects of certain germicides, like carbolic acid, is not capable of exciting inflammation or putrefaction in animal tissues, or wound-exudations, and is harmless in its action upon wounds. Both observation and experiment prove, I think, that, in the overwhelming proportion of cases, the disorders included under the comprehensive term of blood-poisoning depend upon putrefying or septic fluids in a wound gaining entrance to the general circulation. In short, that, if wound-putrefaction can be prevented, we may eliminate from the deaths occurring after surgical operation or injury those which are produced by blood-poisoning in its various forms—pyæmia, septicæmia, and erysipelas. That blood-poisoning occurs independently of external wound may be conceded, but this is exceptional; and, as a broad clinical fact, I think it has been established that, in surgical practice, blood-poisoning, arising from altered secretions in the wound, has hitherto been the ever-present dread in the minds of those who are dealing with wounds, whether created by the knife or by accidental violence. This is the main and all-important difference between subcutaneous injury and an open wound. A simple fracture unites easily and quickly if kept properly adjusted, with scarcely any pain or fever, and almost no risk; whereas a quite similar amount of injury of bone, if associated with external wound, excites fever; there is pain—possibly extensive suppuration and necrosis; the limb may be eventually sacrificed, or, in no small proportion of the cases, the sufferer will lose his life. Now, if it be true that

these formidable consequences depend for their origin on putrefaction of the wound-secretions; that putrefaction depends upon the free access to the wound of minute living particles in the atmosphere; and that, further, by a certain methodic use of germicide agents, these putrefactive changes in wounds and their consequences may be absolutely prevented; then the various forms of blood-poisoning may be erased from the list of surgical diseases.

In addition to this, the advocates of the method allege that the duration of treatment in cases of injury or operation is, in the main, curtailed; that safe and painless healing of wounds is promoted, and the general comfort and well-being of the patient increased; that many operations, too dangerous to be performed, except of absolute necessity, may, with the safeguards now at our disposal, be done without risk; that operations of expediency—that is, operations for the removal of deformity, or desirable for other reasons, hitherto unjustifiable on account of the risk, may now be performed with so much safety that they become matters of routine occurrence. It is not denied that, under what may be called the less exact antiseptic methods of treatment, inflammation and putrefaction may be slight or altogether absent; that wounds unite by the first intention, patients recover well, and operations of various kinds prove successful—the human body is capable of resisting evil influences within certain limits; but what is contended is, that this result is not to be counted upon beforehand as a matter of course; while, with the exact antiseptic method, wound-putrefaction and all its consequences can be prevented with a certainty unknown in any other way. That cases of failure have from time to time occurred is undoubted; but those who are convinced of the efficacy of Lister's method believe these failures arise from some imperfection in the manner of using it; some unconscious neglect of those details, the punctilious attention to which is so essential.

Professor Lister's method has, in his own hands, been from time to time variously modified and improved, and has now reached so high a degree of perfection, that it is one generally employed, and has not, I think, been thus far improved upon by others in any essential feature.

It may now be convenient to state what are the materials used, and the manner of using them. If an ordinary operation is to be performed, the adjacent surface, if covered with hair, must be shaved, then thoroughly washed with a five-per-cent. solution of carbolic acid. The actual steps of the operation are conducted in a carbolised atmosphere, produced by a jet of steam mingling with a five-per-cent. solution of the acid. The sponges employed, the hands of the operator and his assistants,

are thoroughly purified in a five-per-cent. solution, used again and again, during the operation. The instruments are kept ready for use in a solution of two-and-a-half or three-per-cent., which may be also used for washing the wound and the sponges. All bleeding points must be carefully secured, either by torsion, carbolised gut, or carbolised silk, the ends cut short. Too much time can scarcely be devoted to the complete arrest of hemorrhage. The sutures should be both deep and superficial; the former of wire, the latter of catgut, and if there be tension, the button-suture is useful—a simple form consisting of leaden plates, through which the wire passes. The entire surface of the wound should, if possible, be brought into apposition. In order that the bloody serum which exudes may easily escape externally, produce no tension, or separate the wound-surfaces, as well as because of its proneness to decompose, drainage-tubes should be inserted. The principle on which these should act is, to carry the secretion from the deeper parts of the wound, or from any irregularity or recess, by the straightest shortest road to the surface; therefore, they should be of sufficiently large calibre. Many short drains, rather than few long ones, should be inserted. They should be removed so soon as their function is at end—in many cases, at the end of three or four days, and always before they can set up irritation, or act as foreign bodies. Thus used, drainage is of the first importance; it is primary and preventive. The drainage-tubes of Chassaignac served a different purpose—they were intended to provide for the escape of matter already formed, and might be called secondary drains. A sufficient number of tubes having been inserted, the projecting portions are cut off level with the surface, and a layer of protective silk applied to the wound. Over this are placed several layers of carbolised gauze, wrung as dry as possible out of a five-per-cent. solution of carbolic acid, and fastened to the surface with a carbolised bandage—a detail of some importance. Over this again may be applied a layer of salicylic wool; it exercises an elastic equable compression, and fills up any irregularities of the surface, besides forming an aseptic filter at the margins of the dry gauze, which is now applied as an eight-fold layer over all, and secured by a bandage—a piece of mackintosh being interposed between the last layers of the gauze. The dressing should extend two or three, or several hands' breadth, according to circumstances, beyond the extremities of the wound; and in places where there is much body movement, it is convenient to keep the margins of the dressing closely applied to the surface by a turn of an elastic web bandage. Many parts of the body present special difficulties, as the neck or groin. In all cases the first dressing is the most im-

portant one; too much time and care cannot be devoted to it, for on its success depends the whole future of the case. At a first dressing, too much carbolic acid cannot well be used, while at subsequent dressings, the less the carbolic acid comes into contact with the wound the better. The ideal wound-dressing, in short, is that which affords perfect rest, free exit for secretions, no tension, and antiseptic protection. In contused or lacerated wounds, the stage of "cleaning" with the accompanying discharges is reduced by this treatment to a minimum, or does not take place at all. Incised wounds or amputations heal more frequently and readily by first intention, without redness, swelling, or pain; blood-clots, if undisturbed, remain dark coloured and without change, until granulation-tissue enters into and occupies them. Septic suppuration and necrosial cellulitis do not occur. Early union in amputation tends to avert disadvantages which may follow a protracted healing, the retraction of soft parts, or the adhesion of the cicatrix to the bone. Pain, fever, and duration of treatment, are diminished.

A simple plan for the manufacture of carbolic gauze, devised by Professor Bruns, of Tübingen, seems worthy of trial. To prepare one kilogramme of this gauze, four hundred parts of resin and two litres of spirit are required. After completely dissolving, one hundred parts of carbolic acid are added, and eighty of castor oil, or one hundred of stearine. After soaking, the gauze is hung up to allow the spirit to escape. A few minutes suffice to dry it, and then it is packed in air-tight cases. It can be freshly made with great ease. It contains eight or nine per cent. of carbolic acid, while the ordinary gauze contains six or seven. It is stated to be more pliable, less irritating, and cheaper than the ordinary kind. Bruns has prepared a extract, two litres of which suffice for one hundred *metres* of gauze. It is thus portable and applicable for use in military surgery or in country practice.

Recently Dr. Neuber of Kiel has used drains made of decalcified bone, with these advantages:—that, after having served their temporary purpose, the drains melt away, the dressings do not require to be changed for their removal, and very often it happens that a dressing would not be disturbed except to see after the drainage tubes. They are less likely to irritate than india-rubber tubes, are easily prepared by soaking tubes, prepared from healthy ox or horse bone, in a mixture of one part of hydrochloric acid in two of water, for ten hours, and preserving the tubes for use in ten per cent. carbolic oil, after first washing them in a five-per-cent. carbolic solution. Thus treated, wounds may remain, Dr. Neuber finds, without change of dressing from two to four weeks. In large operations, or those where any

bleeding may be expected, Neuber applies a second dressing twenty-four hours after the first. He states that the cases in Kiel recover without pain, fever, or suppuration, and that the wound-surfaces immediately glue together in the greater number of cases.

Recently Professor Maas, chiefly on account of the poisonous effects of carbolic acid, and its volatile properties, has employed a solution of the acetate of alumina, which may be prepared in a strength of 15 per cent. by adding dilute acetic acid to the hydrated alumina. A three-per-cent. solution arrests the development of bacteria, while four or five per cent. of carbolic acid is required for the purpose. Compresses soaked in a two-and-a-half per cent. solution of the alumina are applied, under a spray of the same strength, and a number of amputations, resections, washings out of the larger joints, treated in this way, succeeded admirably in the Klinik at Freiburg. There was very little secretion from the wounds. As an antiseptic, it is perfectly safe, and the cost is inconsiderable. It can, however, only be used in a moist form.

After a long trial of thymol, I found it too feeble an antiseptic to be safely used in any but quite small operations, or when but little secretion was anticipated, while it possesses no particular advantage over carbol. I place but little reliance upon thymol spray, which is only one part in 2000 of thymol. It gave me the impression that it differed but little from simple steam. The rough, disagreeable condition of the skin of the hands, which first becomes very dirty in appearance, and then peels off, caused by immersion in carbolic solution or spray, may be considerably abated by protecting the surface with carbolic vaseline—one part in 10; but it has the disadvantage of making the grasp slippery.

The employment of Lister's method is not at once or very easily acquired; it requires practice, a capacity and patience for detail. It requires, also, that those concerned in the management of a case shall, without reserve, believe in the germ-theory, or act as if they believed in it. The surgeon in charge of the case must either himself examine into and verify everything belonging to the dressing of that case, or must have some one in whom he can thoroughly trust to do it for him. Less than this will not be putting Lister's method fairly to the test. This is a difficulty in hospital practice, where it is impossible to superintend all the details yourself, and where your house-surgeons or dressers may not be sufficiently impressed with the necessity of a minute attention to details. In this respect the German professors have an advantage over us. In every German hospital there are one chief surgeon, and three assistants who hold office for three years, and who are generally much

senior men to our house-surgeons, and the management of the whole surgical practice of the hospital is under the control of one man.—*British Medical Journal, Dec.* 6, 1879, *p.* 906.

39.—ON ANTISEPTIC SURGERY.

By TIMOTHY HOLMES, Esq., M.A., F.R.C.S., London.

I hold it as admitted that active degenerative dangers leading to the total disorganisation of a part, and perhaps to death, may take place without any access of the air and its germs; and, in some of these cases, gangrene follows, and putrefaction has reached its highest limit before the mischief is exposed to the air.

The results of simpler treatment, and especially of what is called the "open method," render it very difficult to accept the germ-theory. If it were true that the atmosphere is laden with these deadly germs, ought not the results of exposing a wound to the air to be not only somewhat worse, but absolutely different in kind from those of the antiseptic method? And are they so? Mr. T. Smith said that, in order to judge of the effects of antiseptic surgery, you should go, not to the well-ordered and healthy hospitals of this city, but to the neglected and foul places which disgrace the medical arrangements of the continent. It was against such a doctrine as this that I tried to raise my feeble and apparently quite ineffectual protest. Hospitals, as we know from ample experience here, can be made perfectly healthy and fit places for the treatment of all kinds of ailments, not by any special system of dressing wounds, for that can necessarily only affect the wounded, who are a minority of the population of an ordinary hospital, nor by any system of fumigation in order to neutralise the foulness of the hospital air, but by strict cleanliness and ventilation, by preventing the air from becoming foul.

The sum of the whole matter is this. There are, I believe, a very large number of practical surgeons who, like myself, are thoroughly convinced of the value of drainage and antiseptic dressings, and who, like myself, are glad to confess their obligations to Mr. Lister's teaching; but who are not convinced by the evidence which he has up to the present time produced of the relative superiority of his own method of applying them. —*British Medical Journal, Jan.* 3, 1880, *p.* 34.

40.—ON THE PREPARATION OF ANTISEPTIC GAUZE.

By Dr. GEORGE BEATSON, B.A.(Cantab.)

In the present paper I propose to speak of the material which has been introduced by Mr. Lister for application to

wounds, so as to serve as an external dressing which will act the part of the healthy skin, and, by preventing the access of putrefactive agencies from without, will allow of healing going on safely and expeditiously. This external dressing is in reality a compound one, inasmuch as it is made up of several materials, each of which has its own particular duty to fulfil, but I will commence with the consideration of the substance which is of the most importance, because it is its action which creates in the vicinity of the wound a germless atmosphere, and disinfects all discharges coming from it. I refer to the antiseptic gauze.

Although the material introduced by Mr. Lister to form an external dressing is called *gauze*, it should not be so named. The word *gauze*, which derives its name from having been originally made at Gaza, in Palestine, is applied of course to any light transparent fabrics, of whatever fibres, woven in an open manner, with very fine yarn. The texture, however, of gauze is different from that of plain weaving, in which the warp or longitudinal threads are always *parallel* to each other. The essential character of gauze weaving is, that between each cast of the shuttle a crossing of the warp threads shall ensue, so that they are twisted in pairs from left to right and from right to left alternately after each shot of weft, thereby keeping the weft threads at equal distances apart and retaining them in their parallel positions. It is the intervals left between the interlacings which cause that degree of transparency which, without such an arrangement, could only result from a looseness of texture incompatible with beauty and utility.

But, if we look at a piece of what is termed "antiseptic gauze," we will at once see that all the threads are parallel to one another, as in plain weaving, and that there is no crossing of the warp threads; so that we are really dealing with a muslin or fine cotton cloth. And this is the case. The material is known in the trade by the term of "book muslin," which is the most open and transparent of the different muslins, and it is so called because the pieces of it, when they are folded up several times upon themselves, as they usually are, are not unlike books with their uncut leaves.

1. *The substances used to impregnate the muslin and render it antiseptic.*—They are three in number, carbolic acid, resin, paraffin, and each one of them is added with a definite object. I will speak of them in the order named.

Carbolic Acid.—This substance has been so fully spoken of in my paper to the Journal for January of this year, that it need not detain us now, beyond saying that the property which makes it so suitable as an ingredient in the external dressing is its volatility, whereby there is created, in the meshes of the

muslin and in the vicinity of the wound, an antiseptic and germless atmosphere, so that all discharges are prevented from putrefying. As regards the variety of the acid used in the manufacture of the gauze, the No. 3 acid is the kind almost universally employed, and it has heretofore answered admirably. Of course the absolute phenol could be used, and I believe some small quantities of gauze so impregnated have been made, and phenol gauze could be obtained by ordering it from large manufacturers, as Messrs. Macfarlan & Co., of Edinburgh, but it would require to be specially made, and its cost would be considerably greater than that of the ordinary gauze, while, as I showed, its antiseptic virtues would be in no way increased.

Resin.—To understand the part that this substance plays in the gauze, we must bear in mind what are its chief properties and its mode of preparation. The ordinary resin or rosin of the shops is the type of a large class of substances which exist chiefly in the vegetable kingdom, and which are generally obtained by incising the bark of trees, when they exude in the form of oleo-resins, which are mixtures of an oil and a resin, and which gradually harden. Thus, it is well known that from the common Scotch fir, *pinus sylvestris*, a resinous juice exudes, which hardens into tears. If these juices are subjected to distillation, the volatile oil comes over, and the resin is left behind. This is the mode of obtaining ordinary resin. Common turpentine, obtained chiefly from the Scotch fir, is distilled, and after the oil of turpentine has passed over, the residue of the distillation consists of ordinary resin. It is run, while liquid, into metallic receivers, coated with whiting to prevent adhesion, and from these it is ladled into wooden casks or moulds. According to the amount of heat applied in the distillation is the variety of the resin; but, before speaking of these varieties, I may say that if the distillation be not carried too far, the product obtained contains a little water, and is the officinal resin which enters into our pharmaceutical preparations, and is employed in the different plasters and ointments on account of its great adhesive and slightly stimulating powers. In appearance, it is a yellowish, brittle, and pulverisable substance, of a faintly terebinthinate odour and taste, somewhat opaque from the water with which it is incorporated, but easily fusible, and burning with a dense yellow flame and much smoke. Another name for this officinal resin is *colophony*, this term being given to a resin obtained from the pines on Mount Gallesus, near Colophon, in Asia Minor. This fact is seen in the French word for resin, which is "Colophane." As the colour of resin depends greatly on the amount of heat used in its manufacture, we have several varieties. Thus, there is the *black resin*, which is

the cooled brittle mass in the state in which it leaves the still; then there is the *yellow, or pharmaceutical resin*, which is the black modified by the action of water; and lastly, there is the *white resin*, which is rendered translucent by being deprived by evaporation of any water it may contain. In fact, the above are only the leading and distinctive varieties of resin, for they are so numerous that they are known in the trade under different brands—A, B, C, &c., each brand including different kinds. Since the abolition of the duty, the American resin has found its way into the market, and it is very much purer than the English, and fetches a better price. A very suitable resin for use in preparing the muslin is the F brand, although Mr. Lister, I believe, recommends the *virgin resin*, which is more expensive, costing about 3d. per lb., and is so called because it is the earliest which escapes from the trees, and contains the fewest impurities. The qualities which recommend this substance for use in the gauze are its insolubility in water (and consequently in the discharges from a wound), its non-volatility, and the tenacity with which it retains the carbolic acid, giving it off very slowly, not only at the ordinary temperature of the air, but even at that of the body. So great is this tenacity, that in a melted mixture of one part of carbolic acid, with five parts of resin, the taste of the acid can hardly be perceived, demonstrating how strongly the resin holds the acid. As a consequence of this, the antiseptic is stored up in considerable amount, and for a lengthy period, owing to the slowness with which it is given off. Further, the insolubility of the resin is an element which comes into play most advantageously, for the liquid discharges soaking into the porous fabric of the muslin do not wash away the substance which holds the antiseptic on which the dressing depends for its efficacy in keeping out the external septic ferments, which are always ready to find access to a wound and set up the putrefactive processes.

Paraffin.—This is the other ingredient of the mixture used for charging the muslin, and, as in the case of the resin, it will be advisable to say something about its properties, and the reason for its introduction. Paraffin is familiar to all of us, in the form of a beautiful white, crystalline substance, which is often made into candles. It was first found by Reichenbach, in the year 1830, who isolated it from the tar of beechwood, and from its chemical inertness (for it was found to resist the action of acids, alkalies, and other substances, and not even to unite by fusion with other bodies), he named it paraffin (Lat. *parum*, little, *affinis*, affinity). It remained more or less a chemical curiosity until about the year 1847, when Mr. James Young obtained it as a by-product in the distillation of an oily exudation occurring in a Derbyshire coal mine. Prose-

cuting his inquiries still further, Mr. Young found that, by the dry distillation of bituminous coal, he got an oily liquid from which paraffin could be obtained. Accordingly, about 1850, he established a manufactory for it at Bathgate, near which the Boghead coal is found; and this was followed later on by similar works at West Calder; and at each of these places new towns have really developed. The extent to which the industry has spread may be realised by the fact that the works at Bathgate cover an area of 25 acres, while those near West Calder are no less than 70 acres in extent. The processes by which the paraffin is obtained are as follows:—The shaly coal is crushed by powerful machines, and thrown into long iron retorts. It is there kept at a dull red heat, the cinders or waste being removed from the lower end of the retorts. By the heat the coal is decomposed, and the oily vapour escapes from the retorts into main pipes, whence it makes its way into condensers, and there becomes a black, greasy, oily liquid, which flows into large reservoirs. Subsequent processes convert this oily fluid into an oil for burning, an oil for lubricating machinery, a liquid naphtha, and the solid white paraffin, these changes being brought about by a series of refinings and agitations with sulphuric acid and caustic soda, by distillations at various temperatures, and also by the action of hydraulic presses. The solid matter left in these pressure bags is of a dirty yellow colour, and is then subjected to various processes, so as to get it beautifully white. Thus it is dissolved in naphtha, cooled and settled, filtered, steamed, melted, and poured into moulds. In this way it is obtained transparent and pure. I have gone somewhat into detail in reference to the preparation of this substance; for, in a recent lecture in the Lancet, Dec. 20, 1879, Mr. Lister has pointed out that it is necessary to have the paraffin pure. Thus, he says:—"I regret to find that a crude form of paraffin is sometimes employed in the manufacture of the gauze. It has the great disadvantage that it acts on the caoutchouc of the mackintosh cloth used in conjunction with the gauze, and soon makes it soft and useless. When pure paraffin is used, the mackintosh will last for weeks together, and is thus, in the long run, very cheap, as well as perfectly trustworthy." This, then, is a point of some importance in the manufacture of the antiseptic muslin, and only perfectly clear transparent paraffin should be used. If it has an opaque and semi-transparent look, it is not suitable for use, as it has not undergone the filtration through charcoal that it ought to have, so as to rid it of impurities. Of course, this purifying process is expensive, and adds to the cost of the material, which is about 9d. per lb.; but there is another point connected with the purity of this substance which should be borne in mind,

and that is, that, with some reservations, the purer the paraffin, the higher its melting point; and, while some of the less pure varieties will melt about 110°, this very pure form does not do so until it reaches a heat of 145°. This circumstance has to be remembered in preparing the mixture for charging the muslin. But, while paraffin has little affinity for other bodies, are there none with which it will unite? Fortunately there are, and, of these, *resin* is one, so that the two may be made to amalgamate together. With what object, then, is it introduced in preparing the gauze? It is introduced to correct that property in resin which makes it so suitable for plasters and ointments—viz., its adhesiveness, but which, under present circumstances, becomes a drawback, for it renders the mixture too glutinous and adhesive. Placed thus intimately in connection with the resin, it might be thought that the paraffin would rob it of some of its carbolic acid, and perhaps part with it readily; but it has been found to have this peculiarity, that it does not blend at all with carbolic acid in the cold, so that its presence in no way interferes with the tenacity with which the resin holds the acid, but only serves to dilute the mixture of the acid and resin, and prevent the too great adhesiveness of the latter. For these reasons, paraffin is a most valuable agent, and this is at once seen when we bear in mind that, as far as we know at present, there is no other substance that behaves so under similar conditions. Thus, as Mr. Lister points out, if we were to make a mixture of 1 part of carbolic acid, 5 parts of resin, and 5 parts of spermaceti, it would be more pungent than one of 1 part acid and 5 parts resin alone, for the spermaceti, taking up the acid, would only hold it very feebly, and would readily part with it to surrounding objects. The objection, then, to such a mixture would not be on the ground of consistency, for it would mix admirably, but it would be more pungent and less stable than one composed of acid and resin alone. Accordingly, the presence of the paraffin in the mixture only renders it somewhat milder. "It seems needful," says Mr. Lister, "to point out this circumstance, because, from want of knowledge of it, modifications of the gauze have been suggested, in which the paraffin has been replaced by other materials, which cannot fail to be disadvantageous" (Lancet, March 13, 1875). In itself, paraffin is in no way an irritant, for it has been successfully used both alone and as an ointment for applying to chilblains, excoriations, and indolent ulcers.

2. *The proportions in which these different substances are used.*—There has of late been a change in their proportions, this change being announced in the Lancet, Dec. 20, 1879. Mr. Lister there says:—"We have improved upon the gauze of late. The proportions we used of the ingredients were originally 1 part of

carbolic acid to 5 of common resin and 7 of paraffin, the paraffin being added to prevent undue adhesiveness. We have now changed these proportions to 1 of carbolic acid, 4 of resin, and 4 of paraffin. By that means we have, in the first place, a gauze dressing with half as much carbolic acid again in it. It turns out that this is not too irritating, and, therefore, that is a great improvement; for the gauze is, of course, more efficacious antiseptically in proportion as it has more carbolic acid in it. From the diminution of the paraffin we have a little more adhesiveness, but that, I think, is positively an advantage. We find it does not cause any serious inconvenience, and, on the other hand, it tends to prevent the disposition of dressings to slip upon the skin: it helps to keep them more securely in place. And then I may mention as a secondary, though not insignificant advantage, that though we thus increase so considerably the proportion of the carbolic acid, which is the most expensive ingredient, we have not added to the cost of the materials as a whole, because pure paraffin is so much more expensive than resin, that by diminishing the proportion of paraffin to the resin, we have cheapened the article more than we have enhanced its price by increasing the quantity of carbolic acid, so that the gauze is rendered slightly cheaper by this alteration of our proportions."

3. *The method employed for carbolising the muslin.*—Various modes of procedure have been and are in use. Thus, the gauze may be very readily prepared by dipping the cloth into a steam-heated trough containing the antiseptic mixture, and then passing it in several layers between a pair of rollers, to remove any excess of the fluid. This is a plan which has been found advantageous by some manufacturers. Prepared in this way the cloth is left with rather less than its own weight of the mixture, and while the individual fibres are all charged with the acid held by the insoluble resin, the interstices are quite open, and allow the material to absorb discharge. Another method is to dip the cloth as before in the heated and melted mixture, and then to squeeze out the superfluous material by a press, with heated metallic plates. Mr. Lister considers that this gives a less uniform result than the rollers. While, however, these plans are very suitable for manufacturers who make large quantities of the antiseptic gauze, they are quite inapplicable if we require to prepare smaller quantities of it, as in the case of a charitable institution using only a moderate amount in the year. With the view of meeting this difficulty, Mr. Lister has detailed a third way of preparing the gauze, which produces an excellent quality, is easily carried out, and does not involve any expensive apparatus. It is in use in the Royal and Western Infirmaries of Glasgow, and also in the New Royal Infirmary of Edinburgh; and the gauze so prepared is pronounced very good and trust-

worthy. I cannot do better than quote Mr. Lister's own description of this method of preparation, as given in the Lancet, March 13, 1875:—"In order to charge the gauze, the paraffin and resin are first melted together in a water-bath, after which the acid is added, and blended by stirring. The object now is to diffuse this melted mixture equally through the cotton cloth, and for this purpose two things are requisite—viz., that the cotton be at a higher temperature than the melting point of the mixture, and that it be subjected to moderate pressure after receiving it. The cotton cloth, a yard wide, is cut into six-yard lengths, and these having been folded so as to be half-a-yard square, are placed in a dry hot chamber, formed of two tin boxes placed one within the other, with an interval to receive water, which is kept boiling by fire or gas beneath, the upper edges of the boxes being connected and provided with an exit pipe for the steam. There is also a glass tube arranged as a gauge of the amount of the water, and the chamber has a properly fitting lid. The bottom of the chamber is strengthened with an iron plate, to enable it to bear the weight used for compressing the gauze when charged. There is a piece of wood, about 2 inches thick, nearly fitting the chamber, covered with sheet lead, so as to make it about as heavy as a man can lift by means of two handles in the upper surface. The weight is heated along with the cotton, and is put first into the chamber so as to leave the cotton loose for the penetration of the heat, which occupies two or three hours. The cotton, when heated, is taken out of the chamber along with the weight, and placed in a wooden box to protect it from the cold. (It would be better to have a second hot chamber for this purpose, since in cold weather the cotton is apt to be too much cooled in spite of the protection of the wooden box.) The heated gauze is then at once charged with the melted mixture of carbolic acid, resin, and paraffin, in quantity equal to the weight of the cotton fabric (or slightly less), and, in order to diffuse the liquid as equably as possible, it is sprinkled over the gauze by means of a syringe, with a number of minute perforations in its extremity, the body of the syringe and the piston-rod having each a wooden handle to protect the hands of the workman from the heat. The syringe is constructed to hold half the quantity of the mixture required for charging one piece of cloth. One folded piece being placed at the bottom of the hot chamber, its upper half is raised and turned aside, and one syringeful is sprinkled over the lower half. The upper half is then put back into position and another syringeful thrown on. The same process is repeated with all the other pieces of gauze, after which the weight is put into the chamber to compress the charged cotton, and the lid applied. An hour or two are then allowed

to elapse to permit the complete diffusion of the liquid, when the material is fit for use. The apparatus above described can be made by a common tinman for about £10." Such is the plan that Mr. Lister advocates for the ordinary preparation of the gauze.

I will now give a short outline of the method of preparing the gauze adopted in the Royal and Western Infirmaries of Glasgow, and I may say here that the ingredients used in the impregnating mixture, as I may term it, are the varieties mentioned in the early part of my paper—viz., No. 3 carbolic acid, virgin resin, and pure paraffin of a high melting point. An order having been received for preparing, say, 500 yards of gauze, the first thing done is to measure off and cut into six-yard pieces five of the webs of unprepared muslin, which usually contain 100 yards each, though sometimes only 80 yards. The steam having been turned on to the chamber the six-yard pieces, folded half-a-yard square, are now placed in it, no pressure being now exercised on them so as to allow the heat to permeate among the different layers. While this heating of the cotton is going on, the resin and paraffin are being melted in the vessel by means of steam. This is usually accomplished in about two and a half hours, but the mixture requires stirring at intervals, as the resin being the heavier of the two substances, would sink to the bottom of the vessel. This same period of two and a half hours is found sufficient for heating the muslin, which is then removed to a wooden box to keep it warm, and the proper quantity of carbolic acid being added to the mixture in the vessel the process of charging the muslin by means of the syringe goes on piece by piece. I may add that the carbolic acid, on account of its volatility, should not be added to the impregnating mixture until a short time before the actual work of charging the muslin commences. The syringe is constructed to hold enough of the mixture to charge one piece of cloth, accordingly one folded piece of muslin is placed at the bottom of the hot chamber and its upper half being raised and turned aside, one half of the syringeful is sprinkled over the lower half of the piece. The upper half of the muslin is then put back into its place and the remainder of the syringeful thrown over it. The process is repeated with all the other pieces of muslin, when the weight is put into the chamber to compress the charged cotton, and the lid applied. About two hours are allowed to elapse, so as to permit of the complete diffusion of the liquid through the muslin, when the material is fit for use. It is then taken out of the chamber, each piece is unfolded and stretched, and being refolded is ready for use. The stretching process improves the gauze, for if put away just as it is taken out of the chamber it is somewhat

damp, the different layers have a tendency to adhere together, which gives the gauze a firm and hard feeling. This is prevented by this simple expedient. As prolonged exposure to the atmosphere renders the gauze somewhat hard and crisp and deprives it of its soft and slightly moist feel, it should be kept closely packed, and as little exposed to the air as possible, consequently a tin box, with a well-fitting lid, is one of the best ways of keeping it so as to prevent it altering its properties. It is also advisable to keep the supply placed in the wards for daily use in tin boxes, for the same reason. This also prevents dust falling on it.

Muslin prepared in the manner just described will be found to have all its individual fibres charged with the mixture, while the interstices are left open and free for any discharges to make their way through them. As to the quantity of the mixture taken up by the muslin, that varies to some extent with the thickness of the material. If it is a very thin cloth, it will take up less than one of a thicker variety, and there are considerable differences in the thickness of the muslins manufactured. Speaking generally, the muslin when charged has less than its own weight of the mixture, in fact some manufacturers put it at about three-fourths of its own weight. The antiseptic gauze, however, is by no means a heavy substance, one yard of it weighing only about one ounce. I do not propose at present to say anything about the manner of using it. That will come in a future paper, but I may say that it has only to be employed as a dressing to recommend itself as a most cleanly and trustworthy material, provided it is prepared as specified above. And it is necessary to be on one's guard. Not long since there was offered to the profession an article which emanated from Manchester, and was termed *carbolised gauze*, and so I suppose it was for a short period in its existence, but as neither resin nor paraffin was used in its preparation, the carbolic acid vapour that had been passed through it soon left it, and it was practically worthless as an external dressing to keep out septic agencies. There are several other varieties of gauze known in the surgical world, such as *Bruns'*, made with resin, castor oil, carbolic acid, and alcohol, *Eilau's*, made with linseed oil, yellow wax, resin, oil of turpentine, and carbolic acid, and *Pohl's*, made with carbolic acid and alcohol, but, as pointed out in an earlier part of this paper, they all labour under the drawback of having other ingredients introduced to take the place of the paraffin, which really are not suitable to act for it, and they cannot fail to be disadvantageous. In Scotland, Messrs. Macfarlan & Co., 17, South Bridge, Edinburgh, are the largest makers of antiseptic gauze; while, in England, it is manufactured on a considerable scale

by Mr. John Milne, Eagle House, Ladywell, Kent. Gauze obtained from either of these makers may be relied on as excellent in every way and quite trustworthy. I may here say that Messrs. Macfarlan & Co. manufacture two kinds of gauze—one the ordinary brown variety and another of a white colour. This last is made in exactly the same way as the other, only the basis of it is the bleached muslin, and all the ingredients used in making it are of the finest description, the resin especially being the white variety. As a consequence of this it is whiter to the eye and much more expensive than the brown kind. The object Messrs. Macfarlan had in making it was to supply a better quality of gauze for private practice, but they tell me that they only make very small quantities of it now, as the brown variety is found to be quite as suitable and as much liked in private as in hospital work. Occasionally, it is noticed that in places the gauze has a darker and browner colour than elsewhere. This deepening of the colour is due to its having been accidentally exposed to greater heat at that particular part, as, for instance, if it was in actual contact with the sides of the hot chamber; as a consequence of this the fibres of the muslin have got a little charred, and there is less carbolic acid at these places. Sometimes this happens to a whole piece of gauze, as, when it is the lowest of the pieces and touching the floor of the chamber. This condition can easily be distinguished from that in which the deeper colour is due to an excess of the antiseptic mixture, for in this latter state of matters the interstices of the muslin will be seen to be filled up also. As regards the price of the antiseptic gauze, Mr. Lister has reckoned that the cost per yard of the materials for charging is about *one farthing*, so that the only other items to be considered are the cotton cloth and the time of the man preparing it. In 1875, when Mr. Lister's calculation was made, the muslin was 1¼d. per yard, so that any institution having its own apparatus could calculate on producing the gauze at a cost of somewhat less than 2d. per square yard. Since then the cotton cloth has been much cheaper, and I understand that at present the antiseptic gauze as prepared in the new Royal Infirmary, Edinburgh, by means of an apparatus similar to the one described above, costs the establishment a little under 1¼d. per yard. Of course it cannot be obtained by the profession at such a low rate, but either Macfarlan's or Milne's gauze can be bought from the Apothecaries Co. here, in boxes of six yards at 3d. per yard, and if large quantities, such as 1,000 yards, are taken, at 2¼d. per yard, net. The white gauze of which I spoke costs 5d. per yard for six yards, and 4½d. per yard in quantities of 1,000 yards.

Seeing that the cotton cloth is the heaviest item of expense in the antiseptic gauze, Mr. Lister proposes that this item may be avoided to a great extent in hospital work by having the dressings washed and recharged with the antiseptic mixture over and over again. In this way he considers that there will be a considerable saving of expense, and he states "that the cotton cloth is then even better adapted for the purpose than it originally was, having lost the slight rigidity caused by the starch or gum used to stiffen the threads before weaving." If such a course is followed there are two conditions, which even Mr. Lister himself admits are essential to make this procedure successful, the first is that the dressings must be *large* ones, so as to repay the trouble of washing and arranging in proper masses for recharging; and the second is, that care must be taken to cleanse the dressings of the substances with which they were originally charged, and of the discharges with which they have been soaked, by pressing them well in the hot water with a suitable wooden implement. I confess that when the matter is looked at from all sides, I am doubtful as to the saving of expense, for there is such difficulty in washing the dressings free from the resin, and the process occupies such a time that I believe new fabric could be purchased for about the cost of the labour expended in cleansing that which has been once used. I am not sure, too, whether there are not other circumstances which forbid this kind of economy being practised. Not only is it a difficult matter to free the old dressings from the resin and other substances, but the discharges that have soaked into them have also to be got rid of. Now, if we knew that every case in which antiseptic dressings were employed was *aseptic*, then I consider there would be no risk, but we cannot know this. In hospital work many putrid cases have gauze dressings applied to them; and in this fact lies an objectionable element, which not even the re-charging with the antiseptic mixture can entirely remove. On these grounds I think a preferable plan is to have the dressings washed and made into bandages, in which capacity they are not likely to be such a source of danger as when forming the material of the external dressing itself. I may add also that the usual cost of re-charging dressings is 1d. per yard, which, added to the outlay for washing, does not leave much room for saving. Lastly, I would just mention that in Holmes' System of Surgery, vol. v, second edition, p. 624, Mr. Lister gives us the interesting information that the idea of the antiseptic gauze was suggested to him by the principles on which oakum proves such a suitable surgical dressing—viz., its porousness, which allows of the discharges being absorbed, while the creasote stored up in its fibres acts as an antiseptic, and disinfects

all the fluids with which it comes in contact. Oakum, as is well known, was employed very much as a surgical dressing during the American War, and was found to answer exceedingly well.

To recapitulate, then, in conclusion, I would direct attention to the following points:—

1. The external dressing on which Mr. Lister relies for keeping out the sources of putrefaction from a wound is a material which he has termed "antiseptic gauze.'

2. This *gauze* consists of cotton cloth of open texture, known in the trade by the term "book-muslin."

3. The substances used for rendering the gauze antiseptic are carbolic acid, resin, and paraffin.

4. Each of these three substances has a distinct object to accomplish, the resin holding the acid with great tenacity, while the paraffin prevents too great adhesiveness.

5. At present the proportions in which these substances are combined for charging the gauze are one of acid, four of resin, and four of paraffin.

6. The plan recommended by Mr. Lister for the manufacture of the gauze is to sprinkle the muslin, by means of a syringe, with the above substances in a liquid state, and then to subject the material to considerable heat and pressure in a proper chamber.

7. Gauze prepared in this way has carbolic acid stored up in its individual fibres, while the interstices are freely open for discharges to make their way through, and thus the porousness of the material is in no way interfered with.

8. When the gauze is applied as an external dressing the temperature of the body volatilises the carbolic acid, which forms an antiseptic atmosphere in the vicinity of the wound and between the meshes of the gauze, thus depriving the air of its septic influences, and preventing putrefaction of any discharges.

9. The antiseptic gauze should be kept closely packed, and in tin boxes, to prevent its becoming crisp and hard from exposure to the air.

10. If it is determined to wash and re-charge the gauze, after it has been used, this should be done under special precautions.

11. It is an open question whether this procedure is a saving of expense, especially now that the original cost of the new material is so much lessened.

12. It was the use of oakum as a surgical dressing which suggested to Mr. Lister the more refined antiseptic gauze.—*Glasgow Medical Journal, March* 1880, *p.* 183.

41.—A NEW ANTISEPTIC DRESSING.

By Dr. HERBERT L. SNOW, Bayswater.

In the Journal for February 28, occurs a memorandum by Surgeon-Major MacNamara, in which the question is asked, "What is the best substitute in military surgery for exact antiseptic dressing?" And lint, soaked in carbolic acid solution, then covered by a layer of cotton-wool, is the substitute proposed. I venture to suggest another, which I think will be found far superior to this, and which is, indeed, in some respects better than the ordinary Listerian applications for ordinary hospital practice. The dressing to which I allude is called "antiseptic marine lint"; the packages I have seen bear the name of T. Westhorp, Falcon Works, West India Road, E. It is practically fine oakum, but differs from the tenax in ordinary surgical use in being more oily, more tenacious, and more thoroughly saturated (to all appearance) with tarry principles.

My experience of it extends to nine or ten operation-cases, chiefly excisions of the breast. I have operated on those under the carbolic spray; have subsequently applied a layer of the "marine lint," moistened with carbolic solution (without any intervening protective), and two or three dry layers of the same over this; outside the whole, a piece of mackintosh and ordinary calico bandages. The cases were dressed again (always under the spray) on the third day; subsequently, at intervals of from five days to a week. All except one united without any suppuration, quite as cleanly and as rapidly as under the complete Listerian plan. In the exception (a case of excision of the breast for cancer), torsion had been used instead of the ordinary catgut ligatures; there was consequently a profuse serous oozing reaching the outer atmosphere; the mackintosh and amount of "marine lint" were not proportionately enlarged; and the method accordingly failed in preventing suppuration, though subsequently good recovery took place.

For ordinary hospital purposes, this "marine lint" has two advantages over the Listerian gauze; the first is, that it acts much more completely as a sponge, soaking up and holding innocuous in its meshes a large quantity of discharge; the second, that it is very much cheaper—the expense being 2d. or 3d. per dressing, in comparison with from 1s. 6d. to 2s. 6d. in the other case.

For military surgery, I am confident that it will prove the best ready application hitherto devised. The sponge-like action is very valuable; and, when applied on the battle-field to wounds, the large amount of tarry principle held in solution is amply sufficient to keep a wound aseptic for several days, even without the useful adjunct of carbolic solution to moisten the first dressings.

My experience of cotton-wool as a dressing for operation-cases is decidedly unfavourable. It filters the air, doubtless; but in practice the surface next the wound becomes "caked" by the discharge; an impervious "skin" thus formed pens up the latter, and frequent dressings are necessary, or bad consequences quickly follow.

In a dangerous and critical operation, it is a good plan to use Lister's method in its entirety for the first two or three dressings; subsequently, when the discharge has lessened, the "marine lint." I need hardly point out the advantages of this latter as a dressing for large burns in the suppurating stage, or as an application to fœtid cancerous ulcers, etc.

So far, I am not disposed to trust this absorbent lint quite so implicitly as the carbolic gauze and protective; but it is far cheaper, less complicated, and readier of application, and in the majority of ordinary operation-cases answers quite as well.—*British Medical Journal, March* 27, 1880, *p.* 476.

42.—SURGICAL STATISTICS.

By JAMES SPENCE, Esq., F.R.S.E., Surgeon to the Queen in Scotland; Prof. of Surgery in the University of Edinburgh, &c.

[So much has been said of late about Mr. Lister's practice that it is only fair to hear the other side. Mr. Spence's paper gives this side very fairly.]

In my present communication, I limit myself to show what results are obtainable by simpler methods, by comparing my statistics with those of Mr. Lister in as nearly as possible similar cases, and to notice some statements and some omissions in Professor Lister's speech which I think require explanation.

In the editorial challenge in the Journal, those who practise simpler antiseptic methods are called upon to produce an equally "long series of the most tremendous operations in surgery." This demand staggered me a little; for, though I have performed successfully a good many serious and difficult operations, such as removal of large deep-seated tumours from the neck, excision of the scapula, and amputations at the hip, etc., I doubted how far these were entitled to be considered "most tremendous." But, as the operations referred to by Mr. Lister himself seemed by no means more "tremendous" than those I could produce, I venture on the comparison.

I shall take, first, *Major Amputations.* Professor Lister takes a period of five years and three-quarters; since, as he says, his antiseptic system has been more perfectly carried out.

During that period, he had eighty major amputations; and, of these, nine died. Claiming the same right, I take a period before the "antiseptic system" was heard of, when I used the very simplest mode of dressing, merely washing the cut surfaces with tepid water or iodine and water, then uniting the margin with metallic sutures, laying the stump on a mackintosh sheet, and covering it with a veil of muslin or thin cotton —a system to which, with some modifications, I have again returned. During that period, out of sixty-three major amputations for disease, there were three deaths, viz.:

	No.	Recov.	Died.
Hip-joint	1	1	0
Thigh	25	23	2
Leg	5	5	0
Ankle	22	22	0
Shoulder (injury) . ..	3	3	0
Arm	3	2	1
Forearm	4	4	0
Total	63	60	3

During the same period, I had twenty-three excisions, including shoulder, elbow, and knee-joint, with only one death.

Of the two fatal cases of thigh-amputations, one is recorded as death from pyæmia on the twentieth day; in the other, the stump healed almost entirely by first intention, then albuminuria from scrofulous kidney began, dropsy supervened, and terminated fatally. In the fatal case of amputation of the arm, the patient, a man of intemperate habits, had received an injury on the elbow six days before admission. The forearm and hand were erysipelatous, gangrene appeared on the next morning, and the arm was amputated close to the tuberosities of the humerus; but he gradually sank, and died a week after the operation. In this case, the septic influences were evidently in action before the operation. Mr. Lister, in speaking of a fatal case in his amputations of the thigh, explains that the patient died of diphtheria, "nine months after the operation, when the cicatrix was almost complete." Cicatrix of a stump almost complete nine months after amputation! Surely that is not the primary union often claimed as one of the advantages of Mr. Lister's system.

I come now to Mr. Lister's elimination of certain fatal cases, so as to found a claim that "no patient died from a preventable disease" (cause?), or, as he complacently puts it, "every patient recovered who had a chance of recovery." I take, first, his amputations at the hip-joint; these were three in number; one, a primary, I consider a very proper case to deduct. The

operation is warrantable in such cases, as a chance for life and to alleviate suffering, though the chance be infinitesimal. I cannot, however, see on what grounds the other two cases are to be eliminated as non-preventable deaths, unless we are to exclude all unsuccessful cases. Was amputation through the continuity of a bone affected with myeloid disease, thus necessitating amputation at the hip next day, non-preventable? Judging from my own experience, I have little doubt that, had amputation at the hip been performed at first, the result would have been favourable. Or why should a case of large fibroma be specially dangerous? Up to my last statistical reports, I had performed amputation at the hip-joint twelve times, viz., five primary amputations, and seven for disease. Of these, five have been successful; one primary, and four for disease. Of the unsuccessful cases of amputation for disease, two were large medullary sarcomatous tumours of the femur. The fatal primary cases may be summarised thus: 1. Extensive laceration of thigh and scorching of abdomen from explosion of an iron flask of gunpowder in the man's trouser's pocket; patient brought from a distance of fifty-six miles, partly by cart, partly by rail. 2. The patient had both limbs crushed by a railway train passing over them. One thigh was amputated above the knee, the other at the hip-joint. He lived for thirty-six hours. 3. One thigh crushed, and injuries to trunk; and the patient had lost blood before being brought to the hospital from Fife; he never thoroughly rallied, and died next day. 4. A little girl; a heavy cart passed over the thigh, crushing it to a pulp, and there was laceration over the other knee (which had some months before been affected by disease of necrosis of the lower end of the femur). There had also been considerable loss of blood before admission to the hospital. For some time, she went on favourably; a small part of the anterior flap sloughed, but the rest of the stump healed. Albuminuria, which had been present when she was formerly under treatment, reappeared, and she gradually sank.

The fatal cases after amputation for disease were three in number. A young gentleman, twenty years of age, suffering from what was supposed to be hip-joint disease, with intense pain and rapidly increasing cachexy (the disease was malignant malacosteon). The bone was so softened by the lardaceous deposit that the shaft of the femur separated on being depressed, and the head and neck had to be dissected from the acetabulum. He was very weak after the operation. On the second day, severe hæmaturia supervened, and resisted all treatment, and he sank exhausted on the fifth day after the operation. The next case was one of malignant tumour of the soft parts in a powerful young man. I urged him, two years

previously, to have the growth removed when it was small, but he refused. When the amputation was at last performed, the growth reached close to Poupart's ligament, the anterior flap was very short, and all muscular tissue was carefully cut away. There was scarcely any blood lost during the operation, and there was no reactionary hemorrhage. When I saw him some hours afterwards, his pulse was of moderate strength, but he sank suddenly next morning, within twenty hours after operation. The last case was amputation for malignant cystic tumour of the femur, performed as a last chance at the urgent desire of the patient. The details of this case are given in my clinical report of operations in the Edinburgh Medical Journal for November and December last, so I need not repeat them here. The operation was all but bloodless, but he died within thirty-six hours. This case was operated on under "the strictest antiseptic precautions, at the patient's request."

Mr. Lister next proceeds to analyse his cases of amputation at the shoulder-joint during the same period. Four of these were primary, of which one died—a railway accident. Anyone with experience of shoulder-joint amputations knows the great difference in the mortality in railway injuries as compared with machine and gun-shot injuries. Out of twenty-one primary amputations at the shoulder in my own practice up to 1875, I find nine cases for gun-shot, machine, and cart accidents, with two deaths; whilst, out of twelve for railway-accidents, there were six deaths; this is evidently due to the complication of railway accidents with other lesions. In one of the fatal results after operation for accident by machinery, the man had been whirled round. There were found effusion of blood in the pleura, and injury of the abdomen; he lived only twenty-four hours. The other fatal case was from hemorrhage ten days after the operation, due to the patient's own imprudence; the stump had healed pretty firmly; and, fancying himself safe, he sat up. In moving about, bleeding came on; and, being in the country, he died before efficient assistance could be procured. In Mr. Lister's case of fatal result after amputation for malignant tumour, if I recollect aright the description of the case by Mr. Watson Cheyne in his thesis for the Syme Fellowship, the case did not remain aseptic before the bleeding. Besides, surely it is possible, by proper examination of a case, to avoid performing amputation at the shoulder for malignant growth when there exists a similar disease in the thigh. I also think that, under amputations of the shoulder for injury, Mr. Lister must have forgotten a secondary amputation for injury in which hemorrhage took place. I was called by one of the dressers, and reached the ward just as Mr. Lister came. The stump had been dressed antiseptically, for the special dressings

were lying about, but the wound was certainly to my senses of sight and smell not aseptic. I understood the man died soon afterwards. In comparing my results with Mr. Lister's, I have claimed the right which he takes of choosing a period during which I practised a particular mode of treatment of the simplest kind. I would not, however, wish it to be thought that I consider that the proper mode of studying statistics. I have elsewhere given my statistics of five hundred and three greater amputations in a different and more extended form, as performed for different causes under various modes of treatment. More recently, in my later clinical reports, I have furnished further statistics in such form that all the causes in operation as affecting the mortality may be judged of.

In regard to *Operations for Ununited Fracture*, Mr. Lister seems to have been unusually fortunate in meeting with ununited fractures of the thigh-bone; for, in a much longer experience, I have only met with two cases, and one of these was rather delayed union than non-union; in that case, the method of subcutaneous section of the uniting medium, and scraping the ends of the broken fragments (Professor Miller's method) with proper adjustment, was sufficient to effect a cure without any constitutional symptoms or risk. In the other case, an old man with badly set and imperfectly united fracture, I cut down on the bone, and, with the chisel, separated and cleared the ends of bones, readjusted them, and treated the case by moderate extension by pulleys and lateral splints. His temperature only on one day rose to 100°, and that several weeks after the operation, due to irritation of the skin caused by the adhesive plaster which was applied below the knee. In consequence of my having introduced, in 1854, a particular method of resection in ununited fracture of the upper arm, I have been called to operate on several such cases, and always without serious constitutional disturbance arising, and only in one case with failure of union. In it, the lower fragment was so short as to be bent forcibly towards the forearm, and could not be kept in apposition with the shaft of the humerus. In ununited fracture or delayed union in the bones of the leg, I have very generally found Professor Miller's method quite sufficient to produce a cure. In two or three cases of oblique fracture with imperfect union, I have cut down upon the bones, drilled and pegged the fracture after separating the opposed surfaces with the chisel; and in these cases I never met with constitutional disturbance. I can only recollect operating on one case of non-union in the bones of the forearm, and that did quite well. In regard to the thigh, Mr. Lister does not state especially the results of his operations as regard solid union of the fracture: the object of the operation. I know that, in one case, the operation was

repeated either once or twice without union resulting. If such repetitions of the operation be counted as separate operations, that would explain the number of cases met with in five years.

There is one important class of operations on bones of which Mr. Lister gives no examples, viz., resection of the whole thickness of a bone during acute necrosis. I have performed resection of nearly the whole length and thickness of the tibia, leaving the periosteum on three occasions, and of half its length in other two cases; whilst, in my last statistical reports, will be found a case in which I removed the whole length of the ulna from the elbow-joint to the styloid process. In all these cases, the bone was reproduced both as to size and form. In these cases, no spray or special method was used. In all, the result was successful, and the constitutional symptoms improved from the time of the operation.

There is also a class of operations of common occurrence of which Mr. Lister does not give his results. Yet it is a class which has important bearings on his "germ theory." I mean *Excisions of Tumours*. Take, for example, excision of the mamma. I find, on referring to my last three Clinical Reports, 1875, 1876, and 1877-78, that, out of thirty-eight excisions of the breast, and a recurrent cancerous mass from the axilla, there were only two deaths. How are we to account for the escape of these cases in which there is a large cut surface exposed to the atmosphere during the operation, and whilst arresting hemorrhage, if the germs floating in the atmosphere be the causes of all the evils attending operations?

I may add that, in private practice during the last six years, I have only lost one case of excision of the breast, though some of my cases have involved division of the great pectoral, and even removal of part of it, and clearing the axilla up to the clavicle. None of these extensive operations proved fatal—nay, more: so generally do such cases heal largely, if not entirely, by primary union under simple treatment, that I have never felt at liberty to test the spray system in this class of operations. What are Mr. Lister's results in excision of the mamma?

I come now to a statement which, as given in the report, requires some explanation. I mean the statement in reference to the treatment of chronic abscesses. Professor Lister is reported as saying: "When a new principle is propounded, I cannot regard these statistics of individual cases as unimportant. *I say, if a case show new pathological facts, one individual such case is worth as much as a million*" (the italics are mine). "I have published numerous cases, for instance, to show that a great abscess connected with disease of the vertebræ may be opened by free excisions (incisions?), a drainage-tube inserted, and strict antiseptic treatment used; and that from that hour

I never had a drop of pus." From what I know of Professor Lister's style of composition, I think he has suffered at the hands of the reporter or printer; but I must assume that his meaning is fairly represented. I will not in this case state the results of my own experience of the Listerian method in chronic abscesses, because the fault might be said to lie with my application of it; but I must ask Mr. Lister to explain his statement in relation to the fact, of which I cannot suppose him to be ignorant, viz., that, when he left the Edinburgh Infirmary, there remained in his wards many cases of chronic abscesses uncured (I believe seventeen cases); and some of these had been upwards of two years under his treatment. Surely, if the cases had been cured and ceased to discharge, he would have dismissed them from a public charity as cured.

During the period I have been trying the Listerian system, I have admitted a larger number than usual of these not very interesting cases. They have done well as regards constitutional symptoms, but not as to cure or arrestment of discharge. I think this (apart from theory), as regards the absence of constitutional symptoms, is one of the best points in the system; but Mr. Lister imperils the chance of its adoption and usefulness by such exaggerated statements. As to Mr. Lister's very peculiar view of the value of isolated facts or supposed facts, he ought to remember the dictum attributed to John Hunter, that "there are more false facts than false theories in medicine." The difficulty is to decide how far the treatment used is the cause of the result. About eighteen months ago, a poor little child, shrivelled, yellow, and dwining, was admitted to my ward with large lumbar and inguinal abscesses. The child was apparently so exhausted, that I had no hope of successful result; but I opened both abscesses freely, without spray, and distended the cavities with the weak carbolic lotion, and, after the fluid had been pressed out, dressed then simply with oiled lint. After the second dressing, there was no discharge. The little fellow rapidly gained flesh, strength, and colour, and left cured. Am I from the result of this individual case to assume that the same treatment will generally be followed by the same result? Is it not rather an exception to a general rule?

Then, I think, Mr. Lister was also bound to notice a class of operations into the performance of which he must have been led by his peculiar views as to the value of individual cases. I mean his cases of suprapubic lithotomy. His first case was an almost miraculous escape, and, founding on it, he is said to have promulgated very decided views: 1. As to the perfect safety of opening the peritoneal sac if antiseptic precautions were used; 2. As to the naturally bland and unirritating character of the urine upon living tissues, and that it only

became irritating from the putrefaction caused by germs introduced by septic or unprotected catheters. Professor Lister knows the results of his subsequent cases, in which the perfect safety obtained by the antiseptic method was put to such a crucial test. I repeat, I think he was bound to notice these cases; many of those who heard his lectures after his first successful case may never hear of the subsequent cases, and thus be led to adopt his views as to the safety of antiseptic suprapubic lithotomy.

I do not enter into the theoretical points in Mr. Lister's speech. As to organisation of blood-clots, whilst I would not like to say with Mr. Lister that effused blood not exposed to air never gives rise to bad consequences, as I have frequently found suppuration occur in such cases, and offensive pus discharged when the collection was opened, yet I know, and have shown more than thirty years ago from experiments on arteries, that, when present, a limited blood-clot does become organised and incorporated with the walls of the vessels. And, in one case, I even succeeded in showing vascularisation of the blood-clot through the medium of the plastic basis at the point of ligature. As to the organisation of carbolised catgut ligature, I am sceptical. Mr. Lister says that any chemist will tell us that it will not dissolve. I reply that I will believe my own eyes rather than "any chemist," for I have seen it soft and jelly-like, even whilst retaining its form; and, in a stump, I have seen the femoral surrounded by catgut ligature on the fourth day after amputation, bleeding whenever compression was removed, showing the catgut had lost its constrictive power; and it was as easily wiped away with the finger as if it had been recent lymph, thin clot, or other semi-fluid matter.

Two things seem to be admitted by Mr. Lister and his followers in the discussion: that other simpler methods than the spray system may be considered antiseptic, and also that the system is limited in its application; that, for instance, it could not be carried out, Mr. Lister says, in cases where sinuses had existed in the vicinity of the joints which were excised. How many cases demanding excision or amputation do we meet with where such complications do not exist?—*British Medical Journal*, *Jan.* 24, 1880, *p.* 119.

43.—MR. SPENCE ON SURGICAL STATISTICS.

By JOSEPH LISTER, Esq., F.R.S., Professor of Surgery in King's College, London.

Mr. Spence's article on Surgical Statistics consists of two elements; one tending to disparage my character as a surgeon; the other calculated to diminish the effect of the statistics which

I lately adduced in favour of antiseptic surgery. The former of these elements I shall take leave to dismiss without further notice than the remark that, as Mr. Spence never did me the honour to witness the practice which he criticises, so that his knowledge of it, beyond my brief descriptions of the cases, must have been derived entirely from hearsay, the exercise of a little charity towards a late colleague might have induced him, in every one of the points to which he refers, to accord a more generous and at the same time a more just interpretation.

But in direct proportion as Mr. Spence succeeds in persuading his readers of his own superiority and of my inferiority as a surgeon, must his argument tell in favour of antiseptic surgery; for if, in spite of such disadvantages, I am enabled, by the application of a new principle, to obtain results superior to his, the fact must speak volumes in favour of the principle.

First, then, of amputations; Mr. Spence produces a table of excellent results from his own practice. But I have first to remark that they are not derived from the period during which mine occurred, but from a time long antecedent. What is the reason for this selection? We can hardly suppose that Mr. Spence took his illustrations from the practice of many years ago because he thought his method of treating wounds then superior to that which he employs at present. We are, therefore, forced to the conclusion that he has selected a set of cases in which good fortune came greatly in aid of good surgery.

Next, I have to observe that the cases which he compares with mine are not similar to them in kind; for he omits primary amputations, which are by far the most dangerous class, and which are included in my numbers. And the manner in which his statement is made is somewhat apt to mislead, although, I am sure, without any such intention on his part. After the mention of my general results of nine deaths in eighty major amputations, he proceeds to make what I venture to regard as a lame apology for the selection of a different period from mine, and then gives his own figures in a tabular form calculated to strike the eye; and it requires a somewhat close reading of the text to notice that these cases are only amputations for disease, and therefore not fairly comparable with mine. In order to institute a just comparison, I must deduct the primary amputations from my own list. These were sixteen in number, with four deaths, leaving sixty-four amputations for disease and secondary amputations for injury, with five deaths; and thus my percentage of mortality is at once reduced from 11.2 to 7.8. But here I must repeat the observations which I made on these cases in the discussion at St. Thomas's. Of the five deaths, two were after amputation

at the hip-joint, followed by death within twenty-four hours after the operation; and, whether Mr. Spence be correct or not in his idea that the patients might perhaps have been saved by better surgery, these cases have no bearing whatsoever upon the question under discussion, which is the relative safety of different modes of dressing. Both succumbed from the immediate effects of the operation. Those two cases, therefore, should be excluded as irrelevant. But I cannot take leave of them without raising a protest against the view which Mr. Spence must be supposed from his remarks to entertain, that every case of myeloid disease of the lower end of the femur should be summarily treated by formal amputation at the hip-joint. We know how greatly these cases differ among themselves as to the degree in which the bone is implicated beyond the obvious tumour; and many a man would be deprived of life unnecessarily, and many another would lose the advantage of an useful stump, if such a doctrine were allowed to prevail.

My other deaths arose from accidental causes wholly unconnected with the operation. One took place in a woman who presented herself with a swelling of no large size under the right deltoid, so extremely soft that I was unable to satisfy myself whether it was a collection of fluid in the bursa or a solid of jelly-like consistence, without an exploratory puncture, which was made antiseptically; a malignant tumour was diagnosed, and the patient was requested to submit to amputation. This, however, she refused to do, and one day tore off the antiseptic dressings, with the result of causing putrefaction in the tumour and an aggravation of both local and general symptoms that convinced her of the necessity of amputation. By taking the flaps chiefly from the skin, I succeeded in entirely avoiding the putrid element. The amputation-wound followed a typically aseptic course; and I regarded the patient as already safe, when, three days after the operation, bleeding took place in very large amount from a tumour of the same nature in the right thigh, and she died of this internal hemorrhage. The *post-mortem* examination showed that the tumour which had furnished the blood, though it involved the greater part of the thickness of a limited portion of the upper end of the femur, was of such very small dimensions that it could hardly have attracted the attention of a surgeon, supposing him to have examined her from top to toe for the chance of so unlikely a complication.

Of the remaining two deaths, one occurred among my amputations of the thigh, which were twenty-six in number. The amputations of the thigh constitute the main glory of Mr. Spence's table; for, though it contains twenty-two cases of amputation at the ankle-joint without a death, yet that opera-

tion, in the hands of a good surgeon, is so eminently safe, that such a successful series is not very remarkable. But his twenty-five amputations of the thigh with only two deaths are certainly fair subjects of just pride, more especially when we learn from the context that one of these cases died of "albuminuria from scrofulous kidney." We must not, however, assume, without more detailed information as to the precise nature of the renal affection, that death would be inevitable at the present day in such a case, supposing strict antiseptic treatment to be employed; for soundness of the excretory organs becomes a matter of much less essential moment when septic agencies are entirely prevented from entering the organism through the wound, and therefore do not require to be eliminated from the system. And, though Mr. Spence tells us that "the stump healed almost entirely by first intention," we must remember that there is here a mighty difference between almost and altogether, more especially as the operation was performed in the old days of septic silk ligatures.

The other cause of death in Mr. Spence's amputations of the thigh, though it almost appears as if put down under the old notion that it formed a sufficient excuse for mortality, is tragical enough—pyæmia on the twentieth day! I say, then, that in that which constitutes the main glory of Mr. Spence's table there is one melancholy blot; while, merely numerically, his twenty-five cases with two deaths are surpassed by my twenty-six cases with one death. But this one death I must claim as a recovery from the amputation; for the patient died of a totally independent cause—diphtheria—when the stump was almost entirely cicatrised, nine weeks after the operation; for the nine *months* which afford Mr. Spence occasion for such sarcasm are, like other inaccuracies to which he refers, a reporter's error.

The other death was in a boy seven years old, after amputation at the ankle. The stump certainly was slow in healing, as must, I fear, now and then be the case under any treatment where we operate through unsound textures in a feeble constitution. But the cicatrising process was, at the end of three months, almost absolutely perfected, when he died of a cause very rare at such a period of life, but confirmed by *post mortem* examination—cerebral hemorrhage. Now, I contend that it is only strictly just to regard that case also as one of recovery from the amputation.

Thus, excluding as irrelevant the two cases of amputation at the hip-joint where death took place within twenty-four hours of the operation, the results of my amputations other than primary may be thus given in a tabular form.

	Number.	Recovered.	Died.
Thigh	26 ...	25 ...	1
Leg	5 ...	5 ...	0
Ankle	16 ...	15 ...	1
Shoulder	1 ...	0 ...	1
Arm	6 ...	6 ...	0
Forearm	8 ...	8 ...	0
	62	59	3

It thus appears that, even in my lame hands, the application of the strict antiseptic principle has led to results surpassing those attained in a specially selected period by an excellent surgeon, who prides himself upon his success in the department of amputation, but who has not adopted the principle.—*British Medical Journal, Feb.* 14, 1880, *p.* 237.

[Mr. Lister proceeds in many other instances to contradict Mr. Spence's view and practice; and Dr. Watson Cheyne, Surgical Registrar at King's College, adds another paper, in which he still more definitely shows that Mr. Spence is not impartial in his statements.]

44.—PROFESSOR SPENCE ON SURGICAL STATISTICS.

By Dr. JOHN BISHOP, F.R.C.S., Edinburgh.

In Professor Spence's article on Surgical Statistics I observe the following statement: "When Mr. Lister left the Edinburgh Royal Infirmary, there remained in his wards many cases of chronic abscesses uncured (I believe seventeen cases), and some of these had been upwards of two years under his treatment."

It will be obvious to all your surgical readers that, unless Mr. Lister had ceased to treat chronic abscesses at a period long anterior to his removal from Edinburgh, he must have left some cases uncured. Mr. Spence's account, however, is greatly exaggerated; and, as I was personally conversant with the circumstances, I beg you to allow me, in justice to Mr. Lister, to state the actual facts. The cases in question were as follows:

Name.	Age.	Disease.	Date of Admission.
William M.	17	Abscess of hip-joint	January 7th, 1876
John D.	18	Psoas abscess	March 16th, 1876
Lizzie T.	19	Psoas abscess	August 1st, 1876
James B.	23	Abscess of hip-joint	January 5th, 1877
Hugh McL.	6	Lumbar and psoas abscess ...	January 19th, 1877
Michael C.	21	Two lumbar abscesses	January 25th, 1877
John B.	25	Psoas abscess	January 25th, 1877
Alex. W.	16	Abscess of hip-joint	June 22nd, 1877

Thus the cases were eight in number instead of seventeen;

and, as Mr. Lister left the Infirmary in August 1877, it will be seen from the dates of admission that only two had been more than a year under treatment, and neither of them so long as two years. Knowing that these cases could have little clinical interest for any one who had not treated them from the outset, and at the same time feeling that they would still demand careful management for a more or less protracted period, Mr. Lister thought it better to remove them from the Infirmary when he went to London. He therefore wrote to ask me to take charge of such cases as the house-surgeon thought it desirable to place under my care. The female patient was sent to King's College Hospital. One male patient, Alexander W., was considered by the house-surgeon unsuitable for removal. He was in a very weak state, some bed-sores having only just healed. He was, however, much improved, and was regarded as in a promising condition. The remaining six patients were transferred to my care, and I had the satisfaction of seeing them all cured with the exception of the little boy, whose abscesses had become putrid before he left the Infirmary, probably from slipping of the dressings owing to the extreme deformity of his body. His father removed him in August 1878 to the West of Scotland. He was then considerably improved, having youth on his side to resist the septic influences; and, when he was last heard of, he was running about.

The results of the rest of the cases were as follows.

William M., healed in December 1877; left in February quite strong. When last heard of, he had gone to Liverpool as clerk in a warehouse.

John D., healed in March 1878; left Edinburgh in May. When last heard of, he was able to walk without support, and was feeling quite strong.

James B., healed in May 1878; left Edinburgh in July. When last heard of, he was working at his trade (as a baker).

Michael C., healed in March 1878; left in May. In July 1878, he was able to go to Peterhead to the herring-fishing. He returned to the fishing in 1879.

John B., healed in December 1877; left in February quite strong and well. When last heard of, he was employed as a colporteur.—*British Medical Journal, Jan.* 31, 1880, *p.* 185.

45.—SUGGESTIONS IN FAVOUR OF A FURTHER DEVELOPMENT OF THE ANTISEPTIC SYSTEM.

By Dr. J. Wilkie Burman, S.S.C.Cantab., Richmond.

The question arises as to whether or not the antiseptic system admits of more intimate application, so that the blood can be previously brought into such a condition, without injury to

health or other drawback, as to contain a ready antidote in case of any absorption of septic matter, and to provide for its speedy and effective neutralisation before further harm is done.

This question can, I think, be fairly answered in the affirmative; for we would seem to have in the *sulphites*, administered internally, a means of previously bringing the blood into such a repelling and neutralising condition, so far as the introduction of septic matter from without is concerned; and this condition it appears can be maintained for some considerable time without apparent injury to the health.

The well-known elaborate researches of Professor Polli, of Milan (who was the first to introduce this subject to notice), seem to afford conclusive evidence as to the great power of sulphurous acid as an internal antiseptic, when administered in the form of sulphite of soda. He found that animals could, without any apparent ill effect, swallow and absorb large doses of the sulphites; and it was observed, as would naturally be expected under such circumstances, that when these animals were killed their bodies long resisted the putrefactive process, and that, whilst an animal killed under ordinary circumstances exhibited rapid and marked signs of putrefaction, an animal which had previously absorbed doses of the sulphites betrayed no signs of putrefaction whatever. Again, in a series of experiments in which no less than 300 dogs formed the bases of the deduction, Professor Polli showed that the sulphites exerted a *prophylactic* and *curative* power when septic poisons were introduced into the economy. Then, as regards the human subject, he found that the stomach would tolerate large doses of the sulphites of soda or magnesia, which "were tried in the various eruptive fevers, intermittents, typhus, typhoid, pyæmia, puerperal fever, dissection-wounds, malarial fevers, &c. The records of cases treated in this way showed an extraordinary amount of success." According to Ringer, moreover, "so powerful is the antiseptic action of sulphurous acid, when introduced into the system, that sulphites administered by the mouth will, it is said, prevent decomposition and putrefaction of urine in the bladder."

Here, then, we surely have an internal antiseptic of great and undoubted value, and which, under the circumstances, would certainly seem to merit a more extended and careful trial, as a prophylactic and curative remedy, than it has yet been recorded as having received in the human subject, in the prevention and rational treatment of septicæmia.

Under the powerful influence of proper hygienic conditions, and the free adoption of external antiseptic precautions, cases of septicæmia after operation are happily becoming rarer and rarer every day; and it is probably only by further attention to prophylactic measures, so far as these may be feasible in the

manner above indicated, that we shall be most likely able to cause them to be rarer still, and perhaps even to successfully and entirely prevent their occurrence. At any rate, there are theoretical and practical grounds for believing that such a pre-induced antiseptic condition of the blood as may be brought about by the internal administration of the sulphites would be highly favourable to the prevention of septicæmia in ordinary surgical cases, and after labour, and in all other conditions in which septic matters are liable to be formed and absorbed; and the adoption of such further precautions, and more intimate application of the antiseptic system, would certainly seem to be indicated, if we are to give our patients every possible chance of escaping danger under such circumstances.

Further experience of the internal use of the sulphites may yet show that the condition of blood thereby induced might have an additional favourable influence on the healing process itself, and experiments might well, in my opinion, be made by those having the necessary facilities to ascertain if such be the case or not.

The successful use of benzoate of soda administered internally as a prophylactic in diphtheria by Dr. Eigenbrodt, on the occasion of the alarming outbreak of that disease in the Grand Ducal family of Hesse-Darmstadt, demonstrates a still wider scope for the use of prophylactic therapeutic agents; and altogether, in these days, when the subject of *preventive* medicine is attracting such great attention on account of its paramount importance, the practice of prophylactic therapeutics seems destined to play a not unimportant part in the programme of the general practitioner.

In conclusion, I would fain express a hope that, in thus redirecting attention to the important researches of Professor Polli, I may be the humble means of stimulating to further careful testing of his results in reference to the human subject, and to further inquiry into the practical value or otherwise of his conclusions, as a result of which I feel confident experience will show that, by means of internal antiseptic medication, we are in possession of powers of control, with regard to septicæmia and kindred conditions of the blood, which may hereafter be extensively used to great advantage in the prevention and treatment of disease.—*Lancet, Nov.* 22, 1879, *p.* 758.

46.—THE DRY COTTON-WOOL PERMANENT DRESSING IN AMPUTATIONS.

By ARTHUR E. J. BARKER, Esq., F.R.C.S.I., Assistant-Surgeon to University College Hospital.

The main principles upon which the method is based are in

no way antagonistic to those which underlie what we know as the antiseptic system. I should be sorry that you should carry away any other idea, believing, as I do, that the patient and temperate advocacy of those principles by Mr. Lister has been of incalculable benefit to surgery (whether right in every detail or not), and has produced an effect which will last as long as surgery is a science.

By absolute cleanliness we mean the exclusion from contact with our wounded surface at any time during its treatment, not only of all palpable organic or inorganic impurities, but also of certain almost invisible minute living organisms, which we know are present in the air, and which, it is believed by many, are capable while alive of producing decomposition in the secretions of wounds. To do this, however, it is necessary, in the first place, that they should reach the wound in a living, active condition, and further, find in the latter a fluid of a certain character and density in which they can propagate largely. It has been shown, for instance, by Professor Nägeli that one of the most, if not the most, dangerous of all these organisms is quite unable to exist long or to propagate in a moderately concentrated organic fluid, in which they will develop and swarm if it be a little diluted. Possibly it is for this reason, as has been pointed out, that where an open wound is only secreting enough lymph to glue its surfaces together, we never find fœtor and decomposition; whereas if the same kind of wound were effusing even serum which was retained about it, it would be fœtid and swarm with bacteria. Where the latter fell upon viscid organisable lymph, they had not sufficient moisture to support life; while this would be supplied by the serum. This is the explanation of a believer in the germ theory of decomposition in wounds, and harmonises with some of Mr. Lister's observations. On this reasoning, if we could obtain a perfectly dry dressing and a moderately dry wound whose secretions should remain above a certain density throughout, the presence or absence of noxious germs would be a matter of indifference, for in such a case they would not be able to propagate. But such conditions could only be very exceptionally met with, and hardly ever in amputations, though they are known.

Now, to avoid both these sources of danger, we must provide some way or other for the free escape of these fluids, whether by drain-tube, strip of lint, gutta-percha, or other substance interposed between the lips of the wound. But we must beware, in placing a foreign body in the latter for the sake of carrying off its secretions, that this body be not present in such amount, in such a way, or for such a time, as itself to produce undue irritation. For it would thus, as I said before, defeat its own object and provoke discharge. This is a point

we are all constantly liable to forget, and do forget frequently, in the use of the ordinary drain-tube. It is often made unnecessarily large, and left in too long. Again, drainage should not be taken to mean merely conveying the secretions out of the wound, but also as far as possible away from the latter, so as to keep it in as dry a condition as may be for the reasons given. Remember always that dry organic substances are less liable to rapid decomposition than moist under similar conditions.

Turning now to the question of rest, we must bear in mind that the term as applied to wounds should have its fullest physiological signification. It should not be merely regarded as the absence of movement of the limb during and after the operation. A wounded part may probably be kept in a state of unrest quite as much by the continuous contact with it of an irritant fluid, whether applied as a dressing or accumulating in its own tissues, by a clumsy or misplaced drainage-tube, or by an unevenly applied bandage, as by being moved about or roughly handled, and the consequent constitutional disturbance may be equal or even greater. If, during the operation, the wound be wiped over with any particularly stimulating solution, be it of alcohol, carbolic acid, or anything else, a very real irritation of the exposed nerve filaments and tissues is induced, and we violate one of the first laws of physiological rest. Cleansing solutions, then, should be as weak as possible, compatibly with the preservation of their special properties. But how much greater is the irritation when we sponge and scrub the face of a wound with strong solutions and with unnecessary zeal, as many are often forgetful enough to do, myself amongst the number. Such treatment is usually followed by copious reactionary outpouring of serum. Some wounds, it is true, have to be energetically excited to promote a decided reaction for special purposes, but not those of amputation, where we mostly have fresh clean-cut tissues. In arresting the primary hemorrhage, then, and in cleansing, stitching, and placing a drainage-tube in the wound, we should usually aim at doing only as much to the raw surface as is absolutely necessary, and no more; and our cleansing solutions should not be too strong or used too long. In applying, also, our first dressing, we should use as much care as if it were going to be left on for weeks. And, if it could be left on for weeks, it would be so much the better. The wound thus at rest would incline to pour out only as much plastic material as would be necessary for union and could be rapidly organised, and would not be excited over and over again at each dressing to give out more fluid material in the shape of serum or pus. And supposing that we regard bacteria as hurtful, a wound

comparatively dry from lack of excitement would offer a very unfavourable soil for their propagation, even if they had access to it. Suppose, then, two dressings perfectly pure in the first instance, that one would manifestly be best for the wound which could be left on longest. This, too, we are often liable to forget in our anxiety to see how all is going on about the part operated on. To the patients, also, it is in many cases of almost incalculable importance that they should be spared, not only the pain of disturbing the dressing, but, often far more, the mental distress they go through at such a time. Pain and anxiety are two powerful depressants of vital force; and it may make the difference of life or death to a feeble individual exhausted by disease whether we have an extra twenty-four hours of rest from disturbance of his wound, and the pain and emotional disturbance consequent thereon. How much better, then, is his state when he can be left entirely alone for days or weeks. Unfortunately, this is often impossible or undesirable with the present modes of drainage. Though it is no longer necessary to meddle with wounds in order to pull away ligatures (thanks to Mr. Lister's invaluable invention of the absorbable catgut—a very great step in the direction of rest), still there is the drain-tube in the wound as a foreign body. For this reason, we are obliged to take down any dressing which includes it, in order to shorten or remove it altogether, no matter whether the dressing itself could be left on otherwise or not. Could this be avoided, it would be most desirable. And it appears possible that it may be so in the future. In a recent number of Langenbeck's Archiv, there is a very interesting article by a Dr. Neuber, on the use of soluble drainage-tubes. In this, he relates his experience with such tubes in a number of cases. They were made of decalcified bone, which he had found by experiment to be the best material for the purpose. Using these, he was able to leave a Lister's dressing, with some extra packing of salicylated wool, undisturbed for fifteen days, when the tube was found to have been absorbed, and its track healed. Now, if these absorbable drain-pipes can be combined with the catgut ligature and suture, we may perhaps be able hereafter with care to put up an amputation-wound once for all in a pure, dry, absorbent material, and leave it so until it is completely healed before removing the first application. This, if achieved, would go near to realise our ideal dressing, combining cleanliness, adequate drainage, and complete rest.

Let us glance now at the dry cotton-wool permanent dressing as a method of wound treatment, and see how much of these three factors it contains. We have all been familiar, no doubt, for many years with the use of this material for injuries of various kinds, and have seen it of benefit.

Knowing what cotton wool often is in hospital, where it is left about here and there—namely, a trap for dust and dirt—I have had all that employed here baked at a dry heat of 300° to 400° Fahr. This, you know, would render all organic matter in it innocuous, and certainly destroy all germs. Now, pure dry cotton wool is well known to be a perfect shield against the access of dust and germs to substances covered by it. I need not allude to the familiar experiments which go to prove this, and that air is completely filtered of all decomposition-exciting impurities (whatever they may be) by passing through it. If this be true, a wound perfectly clean in the first instance, carefully enveloped in perfectly clean cotton wool in sufficient amount to absorb its discharges, ought to remain quite free from contamination from without until the dressing is removed. The wool, so cleansed, was laid out just before the operation on a towel, to the depth of about six inches, and thus ready to be slipped without loss of time under the stump, and lapped over the end and along its upper surface as soon as the sutures were completed. It was broad enough to envelop the limb, and was firmly bound round the latter with a bandage for a long distance up. Now, what measures had we taken that the wound itself should have been put up within the wool in a really clean state? First, the hands of all participating in the operation were well washed in a five-per-cent. carbolic-acid solution, about the properties of which I must assume we are all agreed. The whole limb both above and below the seat of operation was similarly treated, as also all instruments and appliances used during the latter. When this was completed in the usual manner, and the vessels secured with carbolised catgut, the Esmarch's bandage was removed. The wound was then sponged with iced carbolic solution, perhaps, I since think, with unnecessary energy on my part. After this, I wiped it over in the three first cases with five per cent. of carbolic oil before stitching it with antiseptic silk and inserting an ordinary drain-tube. A strip of lint about as broad as the limb, dipped in the same oil, was then laid along the under surface of the stump, over its end, and along its upper surface. Over this, the wool was now quickly folded as described.

These precautions ought to have secured a clean wound. As the spray was not used, the carbolic oil was employed as described. It was thought that it would adhere to the raw surfaces and flaps longer than the watery solution, and so render harmless any dust or invisible dirt which might fall upon them before they were enveloped in the wool. Should the same have fallen on the latter during our manipulations, the oiled lint would meet it, and render it innocuous for the

wound. The dressing so applied was left undisturbed as long as possible. By this, I mean until either the thermometer indicated undue fever, whether from possible tension or toxic absorption, or the dressings began to give off an unpleasant odour.

Our experience, then, in these instances would seem to show that, as regards the securing of mental rest for the patient and physiological rest for the wounded part, this mode of dressing has shown itself admirable.—*British Medical Journal, Jan.* 3, 1880, *p.* 8.

47.—ON THE TREATMENT OF DISEASED JOINTS.

By Professor VERNEUIL, Paris.

Professor Verneuil lately read, before the Société de Chirurgie of Paris, an important paper on the immobilisation and the mobilisation of diseased joints, the following abstract of which will interest our readers. He began by declaring that "a fundamental principle of therapeutics demands, as an essential condition for recovery, *rest for the diseased organ*," and that "a principle in general physiology not less fundamental affirms that *the activity of an organ* is indispensable to its material and functional preservation," and went on to observe that "from these embarrassing and contradictory propositions it follows that the rest which cures a disease may ultimately annihilate the organ; that the activity which keeps an organ alive may prevent its healing when diseased; and that rest and activity are equally useful, *even necessary*, and yet as equally injurious and dangerous."

Brought to bear on the treatment of arthropathies, the above propositions tend to render our therapeutics and practice undecided and confused. And thus some urge that as the prolonged fixation of a joint may so alter its structure as to lead to anchylosis, therefore we must limit the fixation to the shortest possible time; others maintain that rest, rigorous and persistent, is the best cure for an arthritis, therefore prolong the period of rest to the utmost extent, and disallow any attempt at movement. Bonnet of Lyons, after having enclosed the diseased joints in immovable apparatus for a certain time, always took care, when the right moment seemed to have come, to commence passive movements, in order to restore suppleness to the joint.

This mixed practice seems, nowadays, to be almost universally adopted. Surgeons, no doubt, immobilise joints, because they have found out that it is necessary; but they are always pre-occupied by the supposed ill effects of prolonged fixation, and eagerly look out for the moment when they may

recommence the movements *which are to prevent anchylosis.* Now, Professor Verneuil said, anchylosis, in fact, is a ghost, which frightens not only the lay public, the patients, and their friends, but also nearly all general practitioners, and not a few surgeons.

"In my practice and teaching for a long time past I have combated to the uttermost this idea of anchylosis and its prevention by passive movement. Perhaps my views may seem paradoxical; nevertheless I am led on to the discussion by facts. Thus, a child with joint disease was recently brought to me. I applied absolute fixation to the joint. All the pain ceased, swelling disappeared, and recovery was taking place. At the end of some weeks I was asked when it would be necessary to remove the bandages and commence movements. To this I replied that the time was not yet come. Nevertheless, in a short time, the general practitioner, probably urged on by the friends, removed all the apparatus. As a consequence, the benefits then gained were lost, and the lesion progressed. The child was again brought; some excuses were made. I again ordered fixation, and the child is now in a fair way to recover."

The facts invoked against fixation are indeed very few, and only moderately conclusive; if the accusation is true, we ought to be surprised that the proofs are so uncommon. In order to discuss the subject with advantage, we must at least distinguish between healthy and diseased joints, and among the latter we must further establish varieties. First, then, as regards healthy joints, I affirm that there does not exist a single fact which shows conclusively that fixation, however long continued, has ever led to anchylosis. This long-continued fixation may, it is true, give rise to anatomical modifications such as diminution in the extent of the articular surfaces, to a thinning of their lining cartilage, also to a reduction in size of the synovial sacs, of a less abundant synovial secretion, and to functional changes, such as stiffness of the joints and limitation of movements. Hence, not unnaturally, when the necessity for immobilisation has ceased, a certain time will be required for the complete restitution of the articular function. But there is nothing in all this which at all resembles anchylosis. It is only comparable with what takes place in mucous glands which are no longer traversed either by ingesta or by excretions: they do not become obliterated, as was taught by Bichat, but simply reduced in size. Their healthy condition, however, is again established in a few weeks, or at most in a few months, when their function is once more revived. What better example could one have than the bladder in the case of a vesico-vaginal fistula? It becomes reduced to a mere pouch, but again resumes its normal capacity as soon as the fistula is closed. I am well aware that

everywhere autopsies and experiments on animals are quoted; but neither one nor another have completely convinced me. I could show that the various lesions which are revealed are not in any way of the nature to lead to anchylosis, but can be attributed to other causes rather than to the fixation. On the other hand, I might mention the numberless examples of well-known cases in which the joint, for a long time kept immovably fixed, has, notwithstanding, retained its structure and rapidly resumed its functions when permitted to do so. These latter facts are at least as numerous as the opposite ones, and, being more simple, are also more convincing. It is clear either that fixation *alone* suffices to alter a joint, and then it ought always to do so; or there is need of a peculiar predisposition and a suitably prepared soil, in which latter case it behoves us to seek whether this predisposition does not play the principal *rôle*. The learned Professor inclines to this latter view. He admits that at the termination of any arthritis, in the treatment of which fixation more or less prolonged has been made use of, there is a diminution, a suspension, even an abolition of movement; but does not see why this functional suppression should be attributed to fixation rather than to other causes, especially the anatomical lesions present in the joint?

Those who fear anchylosis argue that certain plastic exudations are poured out between the apposed surfaces, which, at first soft, tend to organise and so glue these surfaces together. Fixation allows this process to proceed uncontrolled. But the synovial membrane is not alone altered; the ligaments are also infiltrated and softened. This no doubt cannot be ascribed to the mere fixation, but the fixation allows the process to go on, whereas movement would certainly prevent the subsequent stiffness and shortening which otherwise come on. The cartilage may even be destroyed, and then, if fixation is carried out, the plastic matter which is deposited ossifies, and true anchylosis is effected; whereas movements would at least tend to a more or less movable joint. And moreover, the tendons are apt to get glued together within their sheaths, which is further favoured by long-continued fixation.

After passing in review the varieties of arthropathy, and the difference in their tendencies, he shows that there are some which never lead to anchylosis; while in others fixation may be carried out or not, there will be some interference with movements in any case, but not an anchylosis. Impaired movement is in all cases due to the disease, and not to the fixation.

The pain of certain arthropathies gives rise to reflex muscular fixation. If moderate, this does not lead to any ill consequences; but if excessive or prolonged, if it go on to contracture, it then becomes harmful, and by bearing unduly on circumscribed

portions of the bone, or cartilages, or ligaments, it gives rise to secondary pathological changes of serious import.

In passive fixation, on the contrary, when mechanical means are used, all movements are prevented, the muscles are kept at rest, and a limb is held in its normal position.

After an examination of the various means by which immobilisation is effected, he arrives at the following conclusions:—

Prolonged fixation incontestably modifies healthy joints, but not profoundly either in form or in the structure of their constituent parts, or as regards their ultimate function.

There does not exist, in scientific records, any authenticated examples of anchylosis produced in a healthy joint by mere fixation. The cases hitherto advanced in support of such an idea are capable of another interpretation. On the other hand, there are on record numerous examples of joints which have been kept immovable for long periods, and have regained their anatomical and physiological integrity.

Inflammation no doubt occupies a first place among the causes; and, as it is absolutely proved that fixation is an antiphlogistic of the first rank, it is illogical to think that it produces those effects which it is known to cure.

If, in certain cases, fixation contributes to produce anchylosis, it is not that fixation which the surgeon secures by apparatus, but rather that which is due to the contracture of the periarticular muscles. As much as the latter, which may be called *active*, favours, and indeed provokes articular disorders, by so much the former, which is *passive*, is powerful against them. There is therefore a capital distinction to make between the two varieties of fixation.

Anchylosis, on the other hand, far from being produced in articular disease, is but a rare termination to it; exceptional in strumous arthropathies, a little more frequent in rheumatic mono-synovitis, anchylosis is especially to be feared in suppurative and traumatic arthritis, though no one variety of disease is certain to produce it.

The exaggerated fear, therefore, of anchylosis has caused many practitioners to make grave errors, and has frequently led to the too early leaving off of passive fixation, and the too premature re-commencement of movement.

Mobilisation, consequent on joint-disease, is of two kinds—artificial or mechanical, and natural or physiological—brought about by muscles, either voluntary or otherwise. The former, which anchylophobes use exclusively, is admissible when we have to deal with the rectification of vicious attitudes of limbs, and to treat confirmed anchyloses; but it ought to be rejected as useless, powerless, and dangerous if we would avoid anchylosis. The latter, on the contrary, is of extreme utility if

applied at an opportune moment: with time it accomplishes in a remarkable degree the restoration of the articular function.

He concludes by saying that artificial fixation on the one hand, and natural fixation on the other, are the two principal therapeutic agents in arthropathies: the one combats anatomical lesions, the other restores physiological action. We may assist the former by different means—local, pharmaceutic, or hygienic; we favour the second by electrisation of the peri-articular muscles, practised during the period of fixation, with a view to the prevention of degenerescence.

To combat the inflammation is the best means to prevent anchylosis. As regards surgical measures proper, I know of none better than continued extension, and, in extreme cases, preventive resection.—*Medical Times and Gazette, Oct.* 18, 1879, *p.* 454.

48.—VALUE OF PARALLELISM OF LOWER EXTREMITIES IN HIP DISEASE AND HIP INJURIES.

By THOS. BRYANT, Esq., F.R.C.S., Surgeon to Guy's Hospital.

A double splint, to which I would draw attention, has slowly been built up at Guy's Hospital, under my observation, to meet the different requirements of such cases of disease and injury of the hip-joint as have passed under my care; and, as now completed, it seems to fulfil satisfactorily all the desiderata of surgery in the cases to which it is applicable.

Its *advantages* are—(1) that it is far more comfortable to the wearer than any other splint that I have used or seen used; (2) that it most thoroughly immobilises the limb to which it is applied; (3) keeps up a well-regulated, steady, and persistent elastic extension, where extension is needed; and, what is of far greater importance, (4) maintains parallelism of the lower extremities; for I would draw attention to the fact that it is to the want of this parallelism of the lower extremities that many, if not most, of the deformities following a natural recovery with a stiff joint after hip disease or injury must be attributed.

I do not propose, however, on the present occasion to enter into all these points; indeed it is not necessary, since the first three are only to be determined by experience; and I must ask therefore to have them accepted on my testimony.

I would recall attention to the fact that in a well-made and healthy subject the spinal column is vertical, and that the pelvis is placed at right angles to the spine, the lower extremities being suspended from their pelvic cups as movable pendents. It is well known, too, that in cases of disease of the hip or of injury to the joint associated with stiffness or anchylosis, this normal position is often greatly altered; and that whilst the positions

greatly vary under these circumstances, they may as a rule be resolved into three—viz., those of *flexion*, *adduction*, and *abduction*.

The special abnormal position will readily be recognised in any given case when the patient is placed on the back and the pelvis is brought at right angles to the spinal column. Indeed, this method of examination is the only right one to bring out the true condition of the limb, and should always be adopted.

With respect to the *flexed* position I have little to say: it of necessity causes some shortening of the limb and an antero-posterior curvature of the spine at its pelvic attachment when the patient is erect and attempts to bring the foot to the ground (lordosis); or when he lies down in the supine position and extends the flexed limb; but it does no more—it does not produce a lateral obliquity of the pelvic bar, as does *ad*duction or *ab*duction. Flexion of the thigh upon the pelvis, pure and simple, is however very rare; in most cases where it exists it is combined with either *ad*duction or *ab*duction, and more commonly the former, with some rotation inwards or outwards of the limb.

With respect to *ad*duction I have much more to say, for I think I shall be able to demonstrate that it is the chief cause of deformity in most cases of disease of the hip-joint associated with anchylosis, as well as in a large number of cases of fracture of the neck of the thigh-bone, and that even where, as a result of disease, displacement of the head of the femur or absorption of its neck exists, when of necessity there must be some shortening of the limb, this shortening is increased in the bulk of cases by *ad*duction.

In children, indeed, with acute hip disease the splint, well applied, so securely guards against joint movement that the nurse may turn the child over on to the sound side for purposes of cleanliness, and lift him for the performance of the natural functions with a freedom which to the beholder looks like indifference, yet she may do so with confidence that no harm or pain will be produced.

As a *corollary* I would draw attention to the fact that a little *ab*duction of the diseased limb is no disadvantage, since such a position, by tilting upwards the pelvic bar on the sound side, and thus raising the sound limb, tends to neutralise the shortening in the diseased one, some amount of real shortening generally attending or following hip disease or hip injuries. This abduction, when needed, can readily be maintained by this splint.

I would add also that with the evils of the *ad*duction of a diseased limb the advantages of maintaining parallelism of the lower limbs in all cases of injury of the neck or shaft of the femur is rendered clear.

As additional recommendations of the double splint, let me add that in the many hip cases to which it has been applied I have found it not only to answer every purpose for which a splint can be required, but it has done so in a far more satisfactory manner than any other with which I am acquainted, the splint, indeed, combining in itself all the advantages of every other, with some extra. It is, moreover, as useful after resection of the joint for disease as it is in its earlier treatment.

In cases of fracture of the neck of the thigh-bone it is equally beneficial; and in support of this view let me add that I have discharged from Guy's Hospital thirty consecutive cases of such injury in subjects averaging seventy-four years of age, with useful limbs.

In fractures of the shaft a like good result may be recorded, for out of forty consecutive cases treated with it, in only four—that is, one in ten—was there any important shortening, such as an inch; in twenty of the remainder there was no shortening, and in eighteen there was less than half an inch. In fact, it may with truth be said that there was no practical shortening in thirty-six out of the forty cases.

With these views, and supported by these facts, it will therefore readily be understood why it is that I am in the habit of using this splint in all cases of hip disease, of hip injury, and fracture of the thigh-bone.—*Lancet, Jan.* 31, 1880, *p.* 159.

49.—ON AMPUTATION AT THE HIP-JOINT AND USE OF THE LEVER FOR CONTROLLING HEMORRHAGE.

By RICHARD DAVY, Esq., M.B., F.R.C.S., Surgeon to the Westminster Hospital.

Some few years ago, I drew the attention of surgeons to the *per rectum* compression of the aorta and iliac arteries for the temporary arrest of hemorrhage during the performance of operations, and practically demonstrated the efficacy of the method in cases of amputation at the hip-joint.

The anterior flap is principally fed by the external, the posterior by the internal iliac artery; therefore, the common iliac artery must be compressed to secure both. And Nature has so focussed her blood-courses at the common iliac artery for the nourishment of the lower limb, that it really seems strange that compression of this vessel should not have been earlier practised.

I would insist strongly on the absolute necessity for restraining loss of blood from the posterior flap; much as I respect the femoral artery in the anterior flap, yet more do I beg you not to underrate the large and numerous muscular branches of the gluteal and sciatic trunks in the posterior. In an interesting

discussion at the Clinical Society of London, following a paper read by my colleague, Mr. A. Pearce Gould (British Medical Journal, May 10, 1879), the use of the rectal lever was received with favour, and a comparison was drawn between it and digital compression. I can only state that, after a surgeon has proved practically how very easy it is to prolong indefinitely compression by the lever, he will not care to submit to the fatigue, uncertainty and anxiety of the digital pressure. Nor can I see why the whole thickness of the abdomen should be squeezed, now that it has been shown that the common iliac artery may be compressed, with only the thin rectal wall intervening.

I have personally compressed the common iliac artery in a man suffering from aneurism of the right external iliac artery, aged 60, for twenty minutes, and without chloroform or other anæsthetic. The only pain felt was the presence of a foreign body; showing how tolerant of pressure the upper part of the rectum is. In no case (so far as I am aware) has any blood or stain been seen in the stool, though I have watched carefully for this event.

On anatomical grounds, the situation of the common iliac artery is perfect for the ends of compression; the lever drops between the psoas magnus muscle and the bodies of the lumbar vertebræ, having the spring (sacral margin) of the true pelvic brim as a counter-resistance to the lever, and no large nervous trunks in the way. I tried to pass the lever once on a dead body at the Westminster Hospital, and failed; I dissected this rectum *in situ*, and found syphilitic stricture in it; in short, the gut was tied up like a boy's cracker; this is the only case in which any difficulty has been met with at present.

The lever is turned out of ebony, and varies in length from eighteen to twenty-two inches. Its surface is very smooth and polished; and its ends are rounded off much like the finger tips. The maximum transverse diameter is five-eighths of an inch; the minimum, three-eighths of an inch. The rectal end is graduated to an inch scale, so that the surgeon who applies the lever can at once learn whereabouts may be the end of the rod.

I will end my first heading by enumerating what I maintain to be the salient advantages of rectal compression.

1. Most perfect control of the required artery.
2. Minimum amount of disturbance of the circulatory system.
3. Independence of the respiratory movements.
4. Its general and easy applicability; strictured rectum being the sole obstacle.
5. The pressure applied is so easy to maintain, and the assis-

tant's body so well out of range of the operator that no hurry need perplex the one, nor anxiety the other.

Its application is quite safe in skilled hands, no injury having ever resulted, and but little pain having been suffered.

7. Cheapness and simplicity; illustrating a lever of the first order.

8. The success hitherto achieved by its employment.

I commend this simple method of restraining hemorrhage to the notice of operating surgeons as safe, reliable, and effectual; its future is by no means limited to amputations at the hip, but is applicable to all surgical procedure where it is desirable to check *pro tempore* the blood-current through the aorta or iliac arteries; and I trust that the wide field of practical surgery may yield instances of its varied utility.

[In a case of amputation in the Royal Hospital, Greenwich, Mr. G. W. Armstrong, the surgeon who operated, says:]

On January 22, kindly assisted by Mr. R. Davy, of the Westminster Hospital, Mr. Johnson Smith, Mr. Pink, and Mr. Bampton, the boy was anæsthetised by Dr. Williams. Mr. Davy controlled the hemorrhage by his peculiar method. He introduced a wooden staff, about two feet in length and three-fourths of an inch in diameter, having a bulbous extremity, into the rectum, for a distance of nine inches; and then, using the anus as a fulcrum, compressed the common iliac artery against the pelvis. I removed the limb by antero-posterior flaps, commencing in the wound made for the excision. Fifteen ligatures were applied; and so completely did Mr. Davy's instrument prevent hemorrhage, in both anterior and posterior flaps, that we were obliged to ask him to relax his pressure in order to discover the bleeding points. The total quantity of blood lost was measured, and was only three ounces, this being mixed with pus. The acetabulum and pelvis were examined, but no bare bone could be felt. The flaps were brought together with silk sutures.

The patient was greatly exhausted after the operation, and was almost pulseless. Brandy being freely administered, he gradually rallied, and, after a dose of fifteen minims of tincture of opium, he slept for a time.—*British Medical Journal, Nov.* 1, *pp.* 685, 687.

50.—ON THE CAUSE OF EVERSION OF LIMB AFTER FRACTURE OF THE NECK OF THE FEMUR.

By EDMUND OWEN, Esq., F.R.C.S., Assistant-Surgeon to, and Lecturer on Anatomy at, St. Mary's Hospital, &c.

From the time when this subject was first touched by the great master, Astley Cooper, surgical writers have, for the most

part, been content to adopt the explanation which he gave. These were Cooper's words: "This state (eversion) depends upon the numerous and strong external and rotatory muscles, to which very feeble antagonists are provided." So far as I am aware, no experiments had ever been instituted with the view of determining the relative strength of the muscles which rotate the thigh inwards and outwards, and it is not improbable that Cooper himself based his statement on *à priori* grounds; nevertheless, his dictum has remained an excellent working hypothesis, though its truth has been but little questioned, and still less challenged.

It will be my object to-day to show that the eversion is due solely to the force of gravity; that when the neck of the femur is broken, the limb "tumbles" on to its outer side. I shall affirm that the muscles of external rotation, though greatly superior in number, are inferior in strength to those of inversion; and I shall also call special attention to the fact that, when the femur is broken below the level of the attachments of the numerous small external rotators, eversion of the limb is still the characteristic feature of the lesion.

The muscles of internal rotation are the anterior two-thirds of the glutæus medius, with the tensor fasciæ femoris and the chief part of the glutæus minimus. The anterior part of the glutæus medius has but a scanty origin from the dorsum of the ilium, but it takes a strong attachment from the deep surface of the fascia lata. That there is in these few muscles an immense mass of fibres for effecting inward rotation, an observer can readily assure himself by grasping with the finger and thumb the firm tissue which bellies up between the front of the iliac crest and the great trochanter when he rotates the limb inwards. If eversion were due to the superior strength of certain muscles, how is it that after fracture of the patella the leg is not found firmly flexed upon the thigh, for the flexors of the leg are then free of all opposition? But opposed sets of muscles are not arranged in active physical antagonism, as if struggling in a perpetual "tug of war." Were this the case, the foot would be doubled upon the front of the leg when the tendon of Achilles is ruptured; and when the wrist-extensors are paralysed in lead-poisoning, the hand would be so firmly bent upon the forearm, that the term "drop-wrist" would but feebly express the abnormal state. Now, these conditions do not obtain; and we have no right to assume that the external rotators of the thigh would act the part which has been generally ascribed to them after the neck of the femur has been broken. That the *elasticity* of the muscles, skin, fascia, vessels, and nerves does much in producing shortening and other deformity after fracture, I am prepared

to admit; but I apprehend that we are in the habit of attributing too much influence to the special muscular contraction. Is it the experience of the surgeon that after an amputation, say of the thigh, the divided muscles have retracted so that they have hidden their ends beneath the long-cut skin? Does he not rather find the skin-flaps disappearing? Does he not trim off tendons which would otherwise hang from between the flaps? When the effects of an anæsthetic have passed off, the ends of divided muscles will quiver on being irritated, but they will not coil into energetic contraction. But supposing the small external rotators did try to roll the limb outwards after fracture of the neck of the femur, they could exert but little influence, as the great trochanter has fallen backwards and inwards towards the great sacro-sciatic notch. This condition would, however, offer improved opportunities to the muscles of inversion. It may be remarked that the special adductors are also concerned in everting the limb; then, I would ask, why do not those muscles rather draw the broken limb across the opposite thigh, and so evince their claim to their name? But the special muscular contraction plays no part in the programme. I think that it will be granted that eversion of the limb is an immediate effect of the fracture, that the eversion is to its full extent, and that the shortening of the limb is often the work of time. And if this be granted, we must seek different causes for the eversion and shortening; and so it is. The eversion is due to the action of gravity; the shortening to the elastic recoil of the skin, fascia, muscles, and other tissues.—*British Medical Journal, Nov.* 1, 1879, *p.* 687.

51.—ON EVERSION IN FRACTURE OF NECK OF FEMUR.

By RUSHTON PARKER, Esq., M.B., B.S., F.R.C.S., Assistant-Surgeon to the Royal Infirmary, Liverpool.

With Mr. Edmund Owen I perfectly agree, that the eversion of the lower limb in sleep, death, paralysis, and fractured femur, is equally due to the unresisted rolling outwards of the limb, owing to the suspension of the ordinary muscular functions. We are strengthened by the testimony of Prof. Spence, in the form of an ancient conviction, and by that of Mr. Cooper Forster in his recent conversion. To me, this was once for all fully proved by a friend, when we were resident colleagues in University College Hospital ten years ago. The fact cannot, I think, be disputed, is probably widely known, and should be recollected in differential diagnosis; though, as Mr. Spence truly says, it needs no profound vindication. Eversion is present in most fractures of the pelvis no less than of the femur, including the neck, and is due in all to the

natural falling outwards of the uncontrollable limb, provided the fracture of the neck be unimpacted.

But what is the cause of eversion in impacted fracture of the neck of the femur? This is a point of some interest, because inseparable from a complete discussion, though unnoticed by all the gentlemen who have shared in it, including Mr. Davies-Colley; but one which has been clearly expounded by Bigelow.

Impacted fracture of the neck he has clearly shown to be everted owing to impaction of the posterior wall only, with a mere bending of the anterior wall, or at any rate its non-displacement if actually broken; due to the greater strength of the anterior wall, as any one may see on examining these fractures in dissection, and on preparing sections of the femur according to Bigelow's directions and engravings.

The rare occurrence of inversion in these fractures he equally clearly explains as due to a greater force, sufficient to overcome and impact the anterior wall as well as, and more than, the posterior.

The notion of muscular contraction having anything to do with impacted fracture of any part of the neck is obviously untenable; and the shape, direction, and deformity in the unimpacted fractures are perfectly explicable without it. We never need to take it into account in any fractures except those obviously due to no other cause, such as cross-break of the patella, or olecranon. For instance, the notion of muscular action in Colles' fracture has been conclusively disposed of by Gordon, in his admirable exposition of the cross-breaking action at the lower end of the radius, due to over-extension of the wrist restrained by the inextensible flexor tendons until the bone itself gives way; and I made a perfect Colles' fracture at a first attempt on the dead body on following Gordon's directions.

I quite agree, then, with Mr. Owen, that "muscular action has nothing to do with it," and think the fact that this is so in impacted fracture of the femoral neck helps to justify the notion, which to many is self-evident, that in unimpacted cases it is equally true. I do not think, however, that it is worth arguing about to any elaborate extent with those who do not admit it.

But a word with Mr. Davies-Colley on another matter. In fracture of the neck, shortening is by no means a necessity, as I have had several good opportunities of knowing, either in impacted or in unimpacted fracture. On this point Bigelow says, "This fracture" (impacted, of neck) "is characterised by shortening and eversion of the limb, sometimes so inconsiderable that we are obliged to accept a diagnosis based upon

an almost imperceptible eversion, and a shortening of half an inch or less, by careful measurement."

When I say shortening, I mean "recognisable shortening," whether by careful measurement, or, what I think better, by squaring the anterior iliac spines with the middle line of the trunk, and placing symmetrically the axes of the lower limbs, whether parallel or not. The shortening can then be made out at a glance. So persistently do surgeons expect to find obvious shortening, that I have known the diagnosis suspended, I had almost said declined, for several days, in an unmistakable case, by two men of great experience and undoubted ability.

The diagnosis is really very easy to the surgically minded, especially after a little experience, and most particularly after having failed to recognise the fracture in even a single case. This injury is a very common one, not only in elderly people, but during and before middle life. But to put the exact method of diagnosis into words is hardly more easy than useful, further than to say that, in immediate disablement through a fall on the trochanter, it may be always suspected and generally found. The snapping-off of the head by indirect violence, such as turning in bed, I equally include among those easily diagnosed.

The most important item of all is the treatment, for which Thomas's hip-joint splint is the splint *par excellence;* and the bed must be soft. In firmly impacted fracture, the cure is painless, or nearly so; but the bed must be soft (this must be insisted on). In unimpacted fracture, with shortening, extension should be added, and the shortening will be reduced, and even abolished.

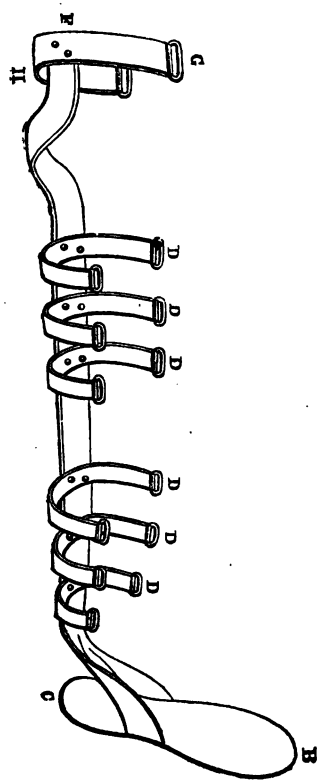
When practicable (which it seldom is), the splint should be put on before moving the patient from the place where he falls; as, in the act of transportation, a limb previously without shortening may be shortened. In fracture near the head, this splint is particularly important (the so-called intracapsular, the separate recognition of which, and its distinction from, so-called extracapsular, I accept Bigelow's opinion in considering an unprofitable attempt at useless refinement). In a few weeks, the patient can get up on crutches, wearing the splint, with a high boot or clog on the sound foot; the supposed inevitable non-union in fracture near the head being prevented, or having the best chance of being prevented.

If non-union should nevertheless occur, a modification of Thomas's knee splint, as an ischiatic crutch with a single stem, will enable the patient to get about on his lower limbs, without the help of his arms.—*British Medical Journal, Nov.* 1, 1879, *p.* 689.

52.—SPLINT FOR EXCISION OF THE KNEE-JOINT.

By Dr. EBEN. WATSON, Senior Surgeon to the Glasgow Royal Infirmary.

[Dr. Watson first published an account of his method of fixing the limb after excision of the knee-joint in the Glasgow Medical Journal for October 1859, which is deserving of fuller recognition by surgeons than it has yet obtained. His splint is described as follows:]



"A B is a firm band of iron on which are fixed D D, &c., bands of flexible tin about three inches apart, and in length about two-thirds the circumference of the limb at each place. A F is another band of white iron joined to the one formerly described at A, and twisted so as to avoid the hip and come up the side of the body as far as the waist, round which the flexible band G H passes about half way, and B C is the foot-piece at the other end. When the splint is applied it is well padded with cotton enclosed in sheaths of gutta-percha tissue, and the foot is bandaged to the foot-piece, while the flexible bands D D D, &c., and G H are pressed in so as to fit firmly to the limb and waist of the patient by means of tapes and buckles. The whole limb is thus kept perfectly immovable, while the apparatus is so light that its position can be easily changed by the nurse without fear of displacement, and any part of the limb may be easily brought into view without disturbing any portion of the apparatus but that at the place required, for the side-bands being flexible have only to be pressed down and the cotton pads may at once be slipped out or opened up as required to expose the limb."

"I have found it best to have no dressings applied to the limb at all except the bandage round the foot and foot-piece and the pads of cotton; so that, with these exceptions, the limb lies naked in the splint." I may further add that now, after excising the knee-joint under carbolic spray, I lay the naked limb in the splint prepared as above on the operating table. The foot is then bandaged to the foot-piece and the side-bands all carefully pressed up and fastened with the tapes. Finally, the antiseptic dressings are placed around the wound, enclosing the splint as well as the limb. Thus, when the limb is again dressed, it is kept fixed in the splint while the dressings are changed under the spray, and very little pain is caused even in the early dressings, none at all in the later ones.

I have treated a considerable number of cases with the above splint, and with great satisfaction, having generally obtained useful limbs; but it must be confessed that complete bony union is sometimes late in occurring, owing, I doubt not, to the previous enfeeblement of the patient by disease. I have tried paraffin and other kinds of splints with the result of only increasing my confidence in the one described.

[Messrs. W. B. Hilliard and Sons, Glasgow, are the sole makers of the splint, which they supply for 21s. complete.]—*See British Medical Journal, Dec.* 13, 1879, *p.* 931.

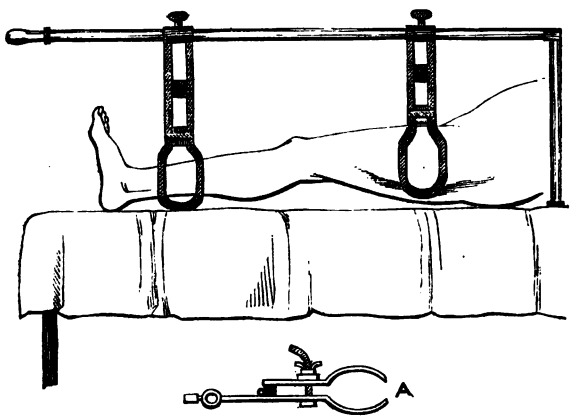
53.—DESCRIPTION OF A SUSPENDING LIFT USED AFTER EXCISION OF THE KNEE.

By HENRY SMITH, Esq., F.R.C.S., Professor of Surgery in King's College, London.

Those who have had the opportunity of carefully watching the course of severe surgical cases where the antiseptic treatment, strictly so termed, has been applied, will be convinced of its enormous influence in bringing about the healing of wounds and in saving life. The observation of some cases which were under the care of my friend and colleague, Mr. Lister, when he first joined our hospital, convinced me of the value of the means he adopted; and in a large proportion of the more severe operations of surgery I follow this method carefully, and the results have been most gratifying.

To no class of cases does the antiseptic plan of treatment so well apply as to those where extensive operations are required about the large joints. This is especially seen in instances of excision of the knee-joint; but hitherto there has been one objection to its application, inasmuch as it was necessary to remove the limb from the splint in which it was confined at the time of the operation, in order that the dressings might be effectually employed. This obvious and forcible objection has

now been done away with through the ingenuity of one of my pupils, Mr. A. Morris, who, having heard me speak about the matter on various occasions, had an instrument constructed which I at once saw would serve the required purpose admirably. At first the clamps were fixed, but on my suggestion Mr. Morris arranged that they should be movable, and this is an advantage. Both Mr. Bell and I have tried the instrument, which I would call a "suspending lift," and we find it well adapted for the object in view. My thanks are due to Mr. Morris for his



ingenuity. The accompanying sketch of the apparatus is by my pupil Mr. Donkin, and it may be described as follows:—

The instrument consists of three parts—a brass rod, an iron upright, and two clamps. The brass rod, including a wooden handle, is 50 in. in length. At the opposite end to the handle, the rod is fixed at a right angle to the iron upright by means of a screw, which admits of slight movement. The rod is cylindrical and hollow. The iron upright is 16 in. in length. The lower end is fixed into the centre of a narrow bar of iron 5½ in. long, which is placed transversely to the brass rod. Four inches from the upper end of the upright there is a curve inwards (not shown in diagram), which allows the brass rod to come over the line of the axis of the leg. The iron clamps are so placed that they can be moved backwards and forwards, and fixed at any point (by means of screws) on the brass rod. The grasping part of the clamp is made on the same principle as the obstetric forceps; a good grip can be made without painfully squeezing the patient's leg. The blades open of their own accord by

means of the simplest steel spring, and are closed by the aid of a nut-screw.—*Lancet, Nov.* 15, 1879, *p.* 724.

54.—ON SUBCUTANEOUS OSTEOTOMY.

By WILLIAM ADAMS, Esq., F.R.C.S., Surgeon to the Great Northern Hospital, &c.

When the subcutaneous method is applied to the performance of operations of unusual magnitude, such as division of the neck of the thigh-bone, division of the shaft of the femur or humerus, it would be unreasonable to expect the uniformity in favourable results which follows the subcutaneous division of tendons, because the conditions upon which the safety of the subcutaneous method depends cannot all be with certainty fulfilled. In these large operations, the air may be effectually excluded: but the injury to the deeply seated structures may be excessive, either from cutting or laceration by the instruments used, or the force employed. Deep-seated suppuration, therefore, will occasionally follow such operations, although the rarity of this result has astonished every surgeon who has practised them. As we recognise the importance of the amount of injury inflicted on the deeply seated structures, in addition to the exclusion of air, it will be obvious that much will depend upon the mode of performing the operation, as well as upon the skill of the operator; and the conditions which must coexist to render the subcutaneous operations free from inflammation and suppuration may be stated to be as follows:

1. That the knife used be of small size, generally from an eighth to a quarter of an inch in breadth, and very little exceeding this in the largest subcutaneous operation, in which the saw or chisel is used, in osteotomy.

2. That care be taken to make an indirect, instead of a direct, track between the cutaneous puncture and the deep structures divided, whether tendon or bone. This may be accomplished by shifting the cutaneous puncture before completing the division of the deeper structures.

3. That care be taken to preserve this track by pressure, which the operator can make with the finger or fingers of his left hand, whenever a second instrument has to be introduced, as in dividing the posterior tibial tendon when a blunt-pointed knife is passed deeply behind the tendon after the withdrawal of the sharp-pointed tenotome; and in all cases of subcutaneous osteotomy, whether the bone be divided by the saw or by the chisel.

4. That the wound be immediately closed on the withdrawal of the knife or other instrument, and a compress and bandage applied, so as to continue the pressure over the seat of operation,

and prevent the effusion of blood under the skin, or amongst the divided structures.

5. That the operation be performed quickly and neatly, with decision rather than force, and with as little disturbance to the soft parts as possible.

6. That perfect quiescence to the part be insured for three or four days, and the dressing remain undisturbed.

When all these conditions are strictly observed, it matters little whether muscles, tendons, ligaments, or bones are divided, or even whether the large joints of the body are opened. There is no better established fact in surgical practice than that subcutaneous wounds seldom inflame or suppurate when the above conditions are fulfilled.

In 1869 I performed the operation of dividing subcutaneously the neck of the thigh-bone within the capsular ligament, with a small saw rather more than a quarter of an inch in width, and with a cutting edge an inch and a half in length, at the end of a blunt shank three inches in length. This saw was passed through the track made by an enlarged tenotomy-knife a quarter of an inch in width. The operation was performed on Dec. 1, 1869, on a man aged 24, a case of bony anchylosis of rheumatic origin seven years previously; the thigh was flexed upon the pelvis at a right angle, and abducted.

I entered the tenotomy-knife a little above the top of the great trochanter, and, carrying it straight down to the neck of the thigh-bone, divided the muscles and opened the capsular ligament freely. Withdrawing the knife, I carried the small saw along the track made—preserving this by pressure of the fingers of the left hand—straight down to the bone, and sawed through it from before backwards. The section was accomplished in five minutes. No hemorrhage occurred; and a compress of lint with plaster and bandage was applied. The limb at once moved freely in all directions; but, before it could be straightened, the tendons of the abductor and rectus muscles had to be divided. No inflammation whatever followed; no swelling or redness of the skin, or any deep suppuration; only a few drops of pus escaped from the granulations at the orifice, which did not close by the first intention, or possibly from the track. Three weeks after the operation, he began to walk on crutches; and by swinging the leg about, as well as applying weight extension at night, I endeavoured to preserve motion; but this was gradually lost, and bony anchylosis in the straight position resulted. The man was enabled to follow his occupation, and now keeps a small general shop at Bath, where he was exhibited to the Surgical Section of the British Medical Association last year.

The decennial period which has now very nearly elapsed since

the performance of this operation has been fruitful in the suggestions of other operations for subcutaneous osteotomy, and also for osteotomy with open wound under Lister's antiseptic method. The operation on the hip-joint has been adopted freely by surgeons in this country, and in America; it is now an established operation in surgery, and if the cases in which it is performed be carefully selected, and the operation executed with the requisite dexterity, its reputation will be maintained. In a paper read at the Royal Medical and Chirurgical Society, Oct. 10, 1876, I recorded twenty-two cases in which the operation had been performed. Cases of a rheumatic or pyæmic origin are the most favourable, because in these affections there is no loss of bone-structure, and the head and neck preserve their integrity; but in the so-called strumous class—unfortunately much the largest—there is generally more or less destruction of the head of the bone, and other alterations in the anatomical relations of the joint; only the most favourable of this class should, therefore, be selected; but for the remaining cases in which my operation is not applicable, Mr. Gant's operation will be found to succeed. Mr. Gant proposed to divide the shaft of the femur subcutaneously, just below the small trochanter, using instruments similar to those employed by myself; but the saw had a longer cutting edge and a thinner blade; the width the same. Mr. Gant performed this operation, Dec. 10, 1872, on a boy aged six, who had anchylosis of the hip-joint, with extreme malposition of the limb upwards and inwards. Primary union of the wound occurred, and firm union of the bone with the limb in a straight position, giving an useful leg.

Mr. Gant has repeated this operation in several cases, and I have also performed it several times. In the same way, I have divided the shaft of the femur in the lower third; and the shaft of the humerus, a little above the elbow joint, in two cases, for bony anchylosis in a straight position. In all the cases, the wound closed by the first intention, without any inflammation, swelling, or redness occurring. The local disturbance seemed to be no greater than in an ordinary tenotomy case.

The largest and boldest operation of subcutaneous osteotomy is that first performed by Dr. A. Ogston of Aberdeen, who applied the principle which I had adopted in the hip-joint to the cure of genu valgum, and used the same instruments, but with Lister's antiseptic precautions, which I have never adopted, as it has always seemed to me that we might absolutely rely upon the protective influence of the subcutaneous law. Dr. Ogston introduced the subcutaneous saw into the healthy knee-joint through the track made by the enlarged tenotome, or subcutaneous knife, and then splitting the lower end of the femur

detached the inner condyle, which in these cases is considered to be in an hypertrophical condition—at any rate, there is no doubt as to its relatively increased length. This operation was successfully performed by Dr. Ogston on May 17, 1876. It has been repeated by many surgeons, both at home and abroad, with great success, and has already become an established operation in surgery. The testimony in favour of this operation by many surgeons is so ample and satisfactory, that it must be regarded as one of the greatest triumphs in subcutaneous osteotomy.

Other surgeons have preferred the use of the chisel, which was most successfully employed by the late Mr. Maunder, who especially interested himself in subcutaneous osteotomy. The use of the chisel in detaching the inner condyle, as it is said, without opening the knee-joint, has been adopted by Mr. Reeves, and with considerable success.

Osteotomy, with Lister's antiseptic method, for genu valgum, has been very successfully adopted by Professor Macewen, of Glasgow; and the operation which he performs is a section, or partial section, of the lower end of the femur with a chisel, without opening the knee-joint.

Professor Lister himself adopts the open-wound, with a free external incision, from one to two inches in length, and then uses the chisel on Macewen's plan. The compound fracture thus made and subsequently treated on the antiseptic method heals, as a general rule, without inflammation or suppuration. The favourable progress of such cases I have recently watched in Professor Lister's practice, but time alone can determine what percentage of unfavourable cases may occur.—*British Medical Journal, Oct.* 18, 1879, *p*, 604.

55.—SUBCUTANEOUS OSTEOTOMY BELOW THE TROCHANTERS.

By Frederick J. Gant, Esq., F.R.C.S., Senior Surgeon to the Royal Free Hospital.

When, in the course of that condition known as scrofulous disease of the hip-joint, the treatment by rest and extension has failed to preserve the integrity of the articulation, or to result in fibrous anchylosis, excision may be resorted to for the removal of the disorganised articular surfaces in order to obtain a fibrous or movable union, and thus an useful limb. But there are cases not a few, where this result is not accomplished by the favourable course of the disease under treatment, or even by the operative interference of excision. The union, at first fibrous, becomes ossific, and, if the patient be at liberty, more or less considerable malposition gradually takes place; the limb being

raised to an angle with the pelvis, so as to be useless for standing or walking. A similar condition is witnessed as the result of chronic rheumatic arthritis; but here the anchylosis is often attended with an exuberant deposit of nodular and hard bone around the neck of the femur.

Under these circumstances, various operations of osteotomy have been devised, having in view the substitution of a fibrous anchylosis, or the formation of a new and movable joint, and thus also to bring down the limb to the position requisite for use. I may refer to the operations originally proposed and performed by Dr. Rhea Barton in 1826, and by Dr. Lewis A. Sayre in 1862. Both these procedures were intertrochanteric—osteotomy between the trochanters; and the latter was an excision of a transverse plate of bone in that situation. Both also were effected by more or less extensive incisions in the integuments.

Subcutaneous section, in the neck of the femur, is the well known operation of Mr. William Adams; first performed by him in 1869, and since practised with considerable success by himself and by other surgeons. This mode of osteotomy has the advantage of an operation governed by the general principles of subcutaneous surgery; and the situation of section, approaching nearest to the centre of motion, is the most desirable for the formation of a movable joint, thus to allow the freest movements of the limb. Accordingly, Adams's operation should always be selected, in appropriate pathological conditions, and when practicable. But there are exceptional classes of cases where this operation is impracticable, or, if eligible, would fail in its result. To these cases I would now direct attention.

Firstly, when, in consequence of continued disease of the hip-joint, the head of the femur has disappeared, leaving only a stunted nodule of bone, representing the neck above the trochanters; in such case, the operation of section in the femoral neck cannot be performed, there being no neck to divide. This advanced degree of destruction may be ascertained by careful measurement of the femur compared with the other. Even when supratrochanteric section is practicable, the state of the neck may render this operation abortive. The seat of operation will be in an almost carious portion of bone, which is unfit to yield a fibrous union; or, possibly, atrophy or necrosis of the upper portion of the neck may ensue, by cutting off the vascular supply from bone already devitalised.

Secondly, another class of cases inappropriate for Adams's operation is when, the anchylosis having resulted from rheumatic arthritis, there is an exuberant deposit of new bone, forming hard nodules or spicula around the femoral neck, itself entire. The thickening and induration existing will resist any justifiable attempt to divide the bone in this situation.

In both these conditions of the hip, the operation of osteotomy may be transferred to below the trochanters; and with this additional advantage, that, the section lying below the small trochanter, the conjoint psoas and iliacus muscles will be set free, and thus the malposition of the limb effectually overcome. For these reasons, pathological and physiological, I originally devised what may be termed the infratrochanteric operation of osteotomy, as an alternative procedure to Adams's operation above the trochanters. My first operation below the trochanters was performed in the year 1872; and the instruments used were a stout tenotomy-knife and a fine saw. These are the only instruments I have since employed: but the late Mr. Maunder preferred Volckmann's chisel, the operation being the same, only performed with a different instrument to divide the bone.

In the most recent case under my care, the condition may be taken as typical of the class wherein, owing to absence of the femoral neck from advanced disease, coupled with immovable anchylosis and extreme malposition of the limb, osteotomy below the trochanters may be practised with the most successful results. The leading particulars are thus reported: "C. K., aged 14, came to the hospital on April 30th, 1878, to see if anything could be done to alleviate the lameness, resulting from old hip-disease on the right side. When standing with the support of a crutch, the tips of the toes on that side just touched the ground, and the limb was dependent so far, only from great anterior curvature of the spine in the dorsal region; for when this lordosis was removed by raising the leg, the thigh formed a right angle with the trunk. The hip-joint had become firmly anchylosed, so that the pelvis moved with the movements of the femur, as though the two were one bone. To conclude the examination, it was found, by careful measurement of the femur, that, shortening having taken place to the extent of two inches, there could be no neck for section in that situation. Mr. Gant performed his operation of subcutaneous section below the trochanters, exactly as described in his Surgery; and, as usual, without any antiseptic precautions. Following the principle of tenotomy, the puncture-wound having been closed with a pad of lint, this was firmly retained by plaister and bandage. Then the limb was brought down to a straight position, and, the section-ends of the femur being in apposition, although with some angular inclination forwards, a long external interrupted splint was applied. The pad was not removed for ten days, when the wound had healed without any suppuration, the lint only sticking to the puncture. At the end of six weeks, the boy was able to get about the ward on crutches, the bone having firmly united; and in three weeks more, the limb could

bear weight with equal balance on the other, and he was discharged from the hospital. A few days ago, after a year's result, he could walk well, with a high heel and thick sole to his boot."—*British Medical Journal, Oct.* 18, 1879, *p.* 606.

56.—A CASE OF OSSEOUS ANCHYLOSIS OF THE KNEE, OPERATED ON BY BARTON'S METHOD.

By MALACHY J. KILGARRIFF, F.R.C.S., Surgeon to the Mater Misericordiæ (late to Jervis Street) Hospital, Dublin.

In faulty anchylosis of the knee, when the angle at the joint approaches or reaches that of a right angle, the crippled limb is useless in locomotion, and not merely useless, but becomes a serious impediment and troublesome burden. It occurred to me that the heroic proceeding devised by Dr. John Rhea Barton in 1826, and afterwards successfully executed in 1838 by Prof. Gibson, and repeated by a few other surgeons, has been lost sight of in this country, and to all intents and purposes has become obsolete. Having myself performed the operation, and being forcibly impressed with the splendid success which resulted, and believing the procedure to be applicable to numbers of cripples left unrelieved and thought to be beyond the reach of surgery, I have deemed it my duty to publish this case.

Case.—M. H., aged thirty, unmarried, the subject of faulty bony anchylosis of the knee, was admitted on the 4th Sept., 1877. Fifteen months previous to admission a fowling piece, in her master's hands, accidentally exploded, the charge shattering the front and sides of her left knee, carrying away the patella and fragments of the femur and tibia. Amputation through the thigh was proposed by the surgeons summoned to her aid, but to this measure she resolutely refused to submit. The injured part was then attended to, and healed after many weary months of suffering. She soon improved in health and strength, but, being unable to walk, owing to the faulty position which the leg had acquired, she was sent to Jervis Street Hospital, to be treated by me.

On examination I found the leg bent at a right angle, the knee immovable, the anterior and lateral aspects being rough and irregular, and covered with a thin slate-coloured cicatrix. With the aid of crutches she could move forwards slowly, but awkwardly—the weight and position of her crippled leg considerably retarding her progress. Resection of the joint I considered inadmissible, there being no skin left to cover the wound which I should have to inflict. I therefore proposed Barton's operation, and the proposal had the full concurrence of my colleagues. Prof. Gibson, and others who imitated and

followed him, cut from the lower portion of the femur a wedge, the base being in front, the thin plate left untouched behind being subsequently fractured in the act of extension. A triangular flap was made of the soft parts in front of the limb, consisting of the integument and the extensor muscles, the lower incision being close to the patella, the base of the flap being to the *inside*.

On the 16th Sept., assisted by my colleagues, I proceeded to operate. Dr. MacSwiney having administered ether, I cut on the lower portion of the thigh a triangular flap, with the base on the *outside*, exposing the femur. I then passed carefully under the bone a curved metallic spatula, and with a saw removed a wedge-shaped piece, which had its base in front, and measuring vertically three-quarters of an inch. I then straightened the limb without difficulty and placed it in a splint made to my order. The outside piece of the splint, like Liston's, reached the axilla, while the inner part fell short of the groin, the lower portion of the apparatus being a box, the thigh part joining that which supported the leg at a *very obtuse angle*. There was on the outside a hinge piece corresponding to the knee, so as to enable the wound to be seen and dressed without disturbing the limb. The dressing, which was applied daily, consisted of lint saturated with carbolic oil, a weak solution of permanganate of potash being also squirted into and on the part to correct the fœtor and prevent septicæmia. She suffered for some days from sickness of stomach, but this complication was successfully met by Schacht's solution of bismuth with prussic acid. The wound suppurated for a few weeks, then closed in, and the divided surfaces of the bone were early united by a firm osseous bond.

In somewhat less than three months she was able to lean on the leg, and, aided by crutches, she could without difficulty walk about the ward. From this onward her progress was continuous and satisfactory; and she was daily paraded for walking exercise before the students, who marked with evident satisfaction her steady improvement. The leg and thigh not being in the same straight line, but meeting at a very obtuse angle, salient anteriorly, she had the advantage of the spring of the foot, which imparted to her gait an elasticity that was truly surprising.

My operation differed from Barton's in two particulars: he left the posterior portion of the bone uncut by the saw—I effected its complete severance; the base of his flap was on the inside—mine on the outside. By cutting quite through the bone, I left no spiculæ to pierce the fascia behind, thus preventing the passage of pus to the popliteal space, and, having the base of the flap on the outside, averted the possi-

bility of the bagging of matter, and made the wound more accessible for dressing.

There was no shortening, or, if any, not more than the tenth of an inch. She is now strong and robust, able to discharge her domestic duties free from lameness or pain, and as favourably circumstanced as the most enthusiastic advocate of Barton's operation could desire.—*Dublin Journal of Medical Science, March* 1880, *p.* 189.

57.—ON SUBCUTANEOUS OSTEOTOMY IN CHILDREN.

By ROBERT WILLIAM PARKER, Esq., M.R.C.S., Assistant-Surgeon to the East London Children's Hospital, &c.

It has long been known that a hypertrophic lengthening of the internal condyle of the femur is the most frequent pathological condition which leads to the production of simple genu valgum. Many other conditions have been assigned. I have myself seen cases in which I could not demonstrate overgrowth in the condyle; in two cases now under care, I believe it is due to overgrowth of the internal tuberosity of the tibia; and I have several times seen how curvatures of the long bones may simulate this deformity. Nevertheless, the broad fact remains, that in a large majority of cases, this undue lengthening of the internal condyle is the cause of the deformity. I am the more disposed to regard this as the real source of the disease, because, in highly marked cases even, after the condyle has been loosened, the deformity can always be overcome.

We owe an especial acknowledgment to Dr. A. Ogston of Aberdeen for having first applied this knowledge to the purposes of surgery; for although other operations had from time to time been proposed and undertaken, none were comparable to Ogston's at the time when it was first published. In recording this, we must not forget to mention the obligation we owe to Listerism; without such a protection no surgeon would have ventured thus deliberately to open the largest joint in the body. In the next place, I think great praise is due to Mr. Reeves for having improved on Ogston, by suggesting a subcutaneous osteotomy; and further, one which may frequently be done without even entering the joint.

Personally, I have not adopted Ogston's operation; I have not even done it once. Reeves' modification seems to me a less serious operation, and just as effective. Since its introduction, therefore, I have given up all other operations for uncomplicated genu valgum. In children, I believe it can be performed without entering the joint, for the layer of encrusting cartilage, together with its synovial lining, would stretch rather than crack sharply off, as was probably the case in adults. Fortu-

nately for my patients, I have had no opportunity of putting this opinion to the test of the *post-mortem* room; but the clinical experience of upwards of twenty-five cases seems to bear me out. Among these cases I have now and then had some effusion into the joint—about three or four times in all. On one occasion, in dividing the condyle, which was very soft, I felt the chisel move too freely, and I anticipated what really occurred—effusion into the joint. Though well marked, it was not really severe, and it quickly yielded to an India-rubber bandage and an ice-bag.

I now generally use the carbolic spray at all these operations, and apply a small Lister's dressing to the wound.

The operation is performed thus. An incision is made with a small scalpel down to the condyle. It should be made about its centre from before backwards; this will correspond to the axis of the femur, and will be about midway between the tendon of the adductor magnus and the inner margin of the patella. The chisel is then introduced, and the condyle is gradually separated. It is well to avoid entering the condyle too far in any one given place; the attempt should rather be made to chisel it equally in each direction by making the handle of the chisel describe the arc of a circle while the mallet is being gently used. If the chisel be sharp, a mallet will not be necessary when operating on young children. After the condyle has been freely loosened, *redressement forcé* is practised until the deformity is corrected. A little over-correction even is desirable. Then plaster-of-Paris bandages are applied from the toes upwards, and the limb is held in position until the plaster is hard. An ice-bag is kept applied for three or four days. There is not much pain subsequently, and rarely any constitutional disturbance whatever. After an interval of from four to eight days, the plaster bandages may be removed, and also the Lister dressing. It will generally be found that the limbs can be *voluntarily flexed to about two-thirds of their extent immediately afterwards.* The amount of stiffness varies with the length of time which has elapsed since the operation. If the joint have not been entered, there will scarcely be stiffness at all. But under any circumstances, I would dissuade from any movements but those of which the patient is voluntarily capable. I should not propose to allow the patient to walk about so soon as this, as it is impossible for the process of union and repair to have been completed thus early. In some of my earlier cases I waited for three weeks, in others for a fortnight, before commencing movements. I have gradually reduced the time, as experience has shown it to be possible. *Voluntary movements* are always to be preferred, and should be practised every day.

Since experience shows that genu valgum does not in any way tend to spontaneous cure, I have in well marked cases not hesitated to operate on children even as young as three years. I have been led on to this by the remarkably little effect which the operation seems to exercise upon them, and by the feeling that it is therefore better to relieve them early in life of a deformity which materially interferes with their own comfort, as well as with that of their families. A child with well marked genu valgum requires very much more care and nursing than another one whose legs are straight and strong. It is the additional labour and responsibility involved in the care of a weakly child which often constitute, for a poor family, the proverbial "last straw which breaks the camel's back." I think, therefore, it is preferable to operate early in suitable cases, seeing how harmlessly it can be done, rather than to wait for two or three years, simply because our patient happens to be a young child.

There is also another argument in favour of an early operation in children when the knee deformity is at all pronounced. It is, that a genu valgum cannot last long without giving rise to a kind of spurious talipes valgus, which then materially complicates the case. In consequence of the obliquity of the leg, the weight of the body falls largely on the inner part of the ankle-joint, especially in the right foot. This unusual and too localised pressure on rickety joints little by little causes the ligaments to yield, and a talipes valgus results, more or less complete in proportion as the knee-deformity has lasted a longer or shorter period.—*British Med. Journal, Oct.* 18, 1879, *p.* 611.

58.—A NEW METHOD OF APPLYING A PLASTER-OF-PARIS JACKET IN THE RECUMBENT POSTURE.

By Dr. J. W. MILLER, Consulting Physician to the Dundee Royal Infirmary.

[It is certainly desirable to have an alternative to the use of the tripod for suspension in the application of a plaster-of-Paris jacket, and this is afforded by the plan of Dr. Walker of Peterborough, published in the *Retrospect*, vol. lxxix., 1879, page 175.]

There is sometimes a difficulty, however, in Dr. Walker's plan, in getting the jacket completed before the plaster has begun to set; at least I have found it so, even with the use of gum to delay the setting. Thinking by what means the procedure might be hastened, the simple expedient suggested itself to me which I am about to describe; of which the great advantage is, that so much can be done before water is employed, that about five minutes' time after the first wetting of

plaster suffices to complete the jacket; this also renders unnecessary the mixture of any substance with the plaster to delay the setting. My method, it will be observed, is only a modification of Dr. Walker's.

The best material for the bandages is Victoria lawn, but not too close in texture; and four yards is a convenient length for them. The plaster should be well rubbed into the bandages, but it is desirable to have a considerable quantity besides loose on the surface of the bandages. Shortly before the jacket is to be put on, the patient should have a full meal, thus putting "a pad" of the correct size inside, instead of Sayre's "dinner-pad" outside. The patient, having put on a thin closely-fitting woollen shirt, is laid on his back in bed, or on a table covered with a folded blanket, a waterproof under him, and his arms extended at right angles to his body. The requisite depth of jacket is marked on the bed by any appropriate means, such as pins inserted in the waterproof or blanket, two on each side, one opposite the axilla and another opposite a point about an inch below the crest of the ilium. The patient is now made to sit, and after doing so must be careful not to change his place on the bed, in order that, when he again lies down, he may resume exactly his former situation. The length of bandage having been ascertained, which is requisite to encircle the body and overlap in front to the extent of four or five inches, pieces of bandage cut to this length are laid down in succession across the bed, each piece overlapping the preceding by two-thirds of its breadth, as Dr. Walker directs, until a sufficient number are laid down to give the required depth of jacket. The first and the last piece laid down should, however, be about a third of their breadth beyond the marks, and should be then folded in, as otherwise the jacket at its upper and lower edges would be only twofold. The ends of the bandages are now fastened together by being fixed on each side between two slips of wood, which are clasped by three American clothes-pins. These slips of wood should be about fourteen inches long, one inch broad, and three-sixteenths of an inch thick, and the wood should be left in its rough state on one side, in order to grip the bandages more firmly. If any of these were to slip out after being wet, the whole operation would be spoiled; this could be effectually guarded against by one of the pieces of wood being pierced by a row of very small sprigs about an inch apart, so that their sharp points might project slightly on the rough side. I have not found this necessary, being careful when lifting the bandages to hold firmly with the fingers. By this means, the whole layer of bandages can be lifted between the two hands and dipped in fluid plaster; but before doing so, a second layer of bandages

must be made ready in the same manner. No water having been as yet employed, all these arrangements can be gone about with perfect leisure, and it may be well to cause the patient to lie down on the arranged bandages to make sure that the correct size has been taken. If the jacket is to be strengthened with slips of tin, these should now be cut to the proper length and laid at hand. Everything being now ready, a thin mixture of plaster is prepared by mixing two pounds in forty ounces of water, and this is then poured on a flat tray broad enough to accommodate the depth of the jacket. One of the layers of bandages is now drawn slowly through the plaster on the tray twice or thrice, care being taken that it is thoroughly saturated, by turning first one side and then the other down; which having been properly done, it is laid aside on any convenient place. The other layer of bandages, having been treated in the same manner, is laid on the bed behind the patient, being carefully placed on the spot previously marked, and the layer which was first dipped then laid upon it. By placing them in this order, the first dipped, and therefore nearest setting, will be first applied to the patient. The patient is now laid back carefully on the bandages, the arms extended as before, and, the slips of wood having been detached from the upper layer, each bandage in succession is brought round and overlapped in front. The slips of wood holding the ends of the bandages firmly between them having prevented the saturation of these portions with fluid, an assistant, while they are being crossed in front, pours on with a spoon some of the fluid plaster from the tray. The second layer is put on in the same way, pieces of tin being interposed where thought desirable. As much as may be necessary of the remaining liquid plaster is now smeared over the surface of the jacket, and smoothed with the hand as it sets. The whole procedure, after commencing to mix the plaster and water, occupies, with ordinary despatch, about five minutes; and on the last occasion, the patient being a child, the whole time occupied, after placing the patient on the bed till the jacket was completed, was fifteen minutes. I have only had four opportunities of employing this method, the last two being by the courtesy of the surgical staff of the hospital. The cases have not been suitable for ascertaining the effect of the recumbent position as compared with that of suspension in straightening the spine and adding to the stature, but Dr. Walker speaks decidedly on this point. The best opportunity of testing the matter would be, in the case of an adult patient about to have a jacket removed, which had been put on during suspension, his height being carefully noted before its removal, and again after the application of a new one in the recumbent posture.—*British Medical Journal, Nov.* 8, 1879, *p.* 728.

ALIMENTARY CANAL.

59.—VALUE OF DEPENDENT POSITION OF THE HEAD IN OPERATIONS ABOUT THE MOUTH AND THROAT.

By PROFESSOR ANNANDALE, Edinburgh.

Eighteen months ago, when removing the greater part of the lower jaw, including its symphysis, I tried the plan of allowing the patient's head to fall over the edge of the table. Although the tongue immediately fell back towards the posterior wall of the pharynx after the attachments of the tongue to the jaw had been freely divided, the man's breathing was perfectly easy—much more so than when the head was raised or lay level with the trunk. Before the patient left the theatre I demonstrated this fact several times to the students present, and thoroughly convinced them and myself of its correctness.

The experience of this case led me to place the head in the same position in my next operation on the throat (thyrotomy); and since then I have performed many operations in this way on the mouth and throat with complete success, and with great facility as regards the prevention of blood passing into the air-passages, the obtaining a good view of the parts, and the carrying out of the necessary manipulations.

Among the operations in which I have used this method have been, a second case of thyrotomy, two cases of tumour of the palate, one case of large epulis, and three cases of cleft palate. In all these operations I have been impressed with the advantages of this position of the head. Complete anæsthesia by means of chloroform, or a mixture of chloroform and ether, has been kept up without any inconvenience during the whole proceedings.

My present method of keeping the head in this position is to have it hanging over the end of the table and supported there by the hands of an assistant; but I am having a little addition made to my operating table which will allow the head to be supported in this position more efficiently.—*Lancet, Nov.* 8, 1879, *p.* 685.

60.—USE OF CLAMP AND CAUTERY IN THE TREATMENT OF HEMORRHOIDS AND PROLAPSUS.

By HENRY SMITH, Esq., F.R.C.S., Professor of Surgery in King's College, and Surgeon to the Hospital.

The operation consists in drawing down and embracing the tumour within the blades of the clamp; then removing the hemorrhoid with a sharp pair of scissors, and, lastly, applying the actual cautery freely to the cut surface.

It is very important to isolate the tumours well, so as to compress them easily, and in some cases where the hemorrhoid runs, as it were, abruptly into the surrounding hypertrophied skin, it is well just to make a slight groove with a pair of scissors. By this means the blades of the clamp compress the growth more readily at its neck. In large tumours I use a clamp furnished with a light but powerful screw near its handles. By this means I tighten the grasp of the blades of the clamp more firmly; then, in removing the part, I take great pains not to divide the base of the tumour too close to the clamp. I leave tissue to the extent of an eighth of an inch, so as to have a fair surface for the use of the cautery. The iron is applied very carefully and very freely at a black heat, and then, all the cut surface being so treated, the blades are gently and gradually released by the screw on the clamp. In this way the pressure is taken off the neck of the tumour, and if any vessel is not thoroughly cauterised it immediately bleeds. The blades are then quickly screwed up again, and the hot iron is applied to the vessel until it ceases bleeding. Sometimes it is necessary, as in a case of huge prolapsus, to reapply the hot iron several times to the same vessel before bleeding ceases. You may, however, rest assured that with proper care you can stop bleeding from any vessel here by the use of the cautery.

Each tumour, and there are generally three or four, is treated in this way, and then, when an opiate suppository is introduced by the finger well oiled, the operation is completed. If there is much hypertrophied skin I remove a portion of it, and if the sphincter ani is very tight, I divide it, as in fissure of the rectum. I do this because I fancy in these cases there is less pain after the action of the bowels; for I find that in those cases of long-standing hemorrhoids or prolapsus, when the sphincter is very loose, the patient as a rule complains very little of pain after the action of the bowels.

The after-treatment consists in locking up the bowels by occasional opiates, and strictly dieting the patient. In the majority of cases the patient complains of more or less severe pain for an hour or two, and then it goes off. This pain is much alleviated by the application of hot water by means of a sponge. Retention of urine sometimes occurs, but not very frequently, more often in women, who generally suffer much more severely from hemorrhoids than do men. As a rule, the patient leaves the hospital in a week, the bowels being acted upon by castor oil three clear days after the operation, and then again two days subsequently. Sometimes blood in a coagulated form comes away with the first movement of the bowels, and frightens the patient, but it is only for the most part blood

which ran into the bowel at the time of the operation and remained there. At times there may be a little arterial bleeding, but I have never once had to use any stronger measures than to apply a speculum and throw up iced water for stopping any such bleeding.

Now the advantages I claim for this operation are—that it is all completed at once; that it is eminently safe; that the after-suffering is slight, and that the period of convalescence is short. In the old-fashioned method of applying ligatures, the operation may truly be described as not completed at one sitting, for of necessity the threads are left to come away, not rarely a most tedious and painful process. I have seen the most horrible suffering for hours and days after the use of ligatures. Then, again, we may meet with those most uncomfortable sequelæ as the result of the presence of foreign bodies, and the subsequent ulceration, tetanus, diffuse inflammation, and pyæmia. Several cases of death have occurred after the use of the ligature from these and other causes; one or two, I regret to say, in my own hands. Some of these cases have been made known, whilst others, especially those occurring in private, have not been heard of. Now, ever since I began the clamp and cautery treatment in this hospital, in the year 1864, I am happy to say we have not had a single fatal or really serious result; and in my own private practice, where during the same period I have operated on several hundreds of cases—some of the most severe description, and at all ages up to eighty—I have only had one death which could fairly and honestly be placed to the account of the operation itself, and this resulted from an accident which ought not to have happened, and occurred several years ago. Three other cases, it is true, died after I had performed an operation, but in these instances, all of which have been published, the condition of the patients was such that it would be very unfair to attribute the fatal result to the operation alone.

I was afraid, however, I should at least meet with a fatal result at the hospital, and this leads me to call your attention to that case of huge prolapsus in which I operated a few weeks ago. The patient was an elderly man who had a prolapsus as big as a cocoa-nut always coming down, and rendering his life a burden. He had already been operated upon twice by a hospital surgeon, but in vain. The patient was then sent to me, and, formidable as the case looked, I determined to undertake it. I applied the clamp deeply in three different directions. There was a great deal of bleeding, and I had to apply the cautery over and over again before I could stop it; and then, just as I was finishing the operation, a most untoward event occurred—severe vomiting, as the result of the anæs-

thetic, took place. The prolapsus was forced still further down; and before I and my assistants could return the parts, the violent action of the abdominal muscles was such that the weakened coat of the bowel gave way, and a knuckle of small intestine actually protruded through the rent thus made. I carefully returned this as soon as the vomiting ceased, and anxiously awaited the result. Our house-surgeon, Mr. Newmarch, watched the patient with great care and treated him with great skill, keeping him constantly under the influence of opium, and locking up his bowels for several days. The result was not a single bad symptom of any kind. On the first action of the bowels there was no protrusion, nor afterwards; and as soon as the man was fairly recovered I removed three longitudinal folds of skin from the anus, so as further to tighten the parts. The man was completely cured. Now, the lesson this teaches is this—not to employ an agent which could cause vomiting; because, of course, in such a terribly severe case as this it is absolutely needful to clamp deeply, and thus weaken the bowel. It was a most unlooked-for accident, not likely to occur again; in fact, it is hardly reasonable to expect to meet with another such case for operation. I have, however, been called to cases as bad or worse, but where no operation could be recommended.

Simple and safe as is the operation by the clamp and cautery, it has gone through an ordeal of severe criticism; and although it has for some time been established as the best proceeding for hemorrhoids and prolapsus, I may refer to the objections advanced against the plan. First, it has been stated that it is a rough and painful treatment; but this argument does not hold good now anæsthetics can be used. A more solid objection is, that hemorrhage of a serious nature is liable to occur. I admit this. I am sorry to say I have known of cases where most serious bleeding has occurred, and I have been informed of two instances where death ensued from this cause; but I maintain that when such accidents occur it is in consequence of an error on the part of the surgeon. He either uses a faulty clamp or allows a vessel to recede from the blades, so that it does not get touched by the cautery, or he applies the cautery inefficiently. I became a little careless after I had operated on a great number of cases, and did not take sufficient trouble to thoroughly cauterise the vessels, and I had about half-a-dozen cases of somewhat severe bleeding; but since then—now some five years ago—I do not let a single vessel escape without being well cauterised, and I have not been troubled by hemorrhage at all. Stricture of the bowel may occur, but this is due to the too free removal of tissue, and especially of the skin about the anal orifice, and of course may be prevented. As a rule, after very severe operations, I advise the patient to use a bougie for

a few weeks; this is a protection against any possible contraction.—*Lancet, March* 13, 1880, *p.* 393.

ORGANS OF URINE AND GENERATION.

61.—CATARRH OF BLADDER; ALBUMEN IN THE URINE.

By Sir HENRY THOMPSON, Bart., F.R.C.S., &c.

There is a group of symptoms frequently met with in men of advancing years, to which I desire especially to call your attention. One of the first circumstances to attract notice is that the urine is more or less cloudy when passed, and that on standing it deposits some adhesive opaque matter in the bottom of the vessel. Such urine is generally neutral, or at best faintly acid; occasionally it is alkaline, always becoming so rapidly by keeping. On interrogating the patient, you learn that the act of micturition is performed rather more frequently than natural —that he is disturbed by it two or three times in the night, and every two hours or so during the day. He may also complain of dull pains about the pelvis and back; he finds the effort to pass water rather greater than it formerly was, and the general health has of late suffered a little.

Now it is by no means uncommon to hear this group of symptoms spoken of as indicating the presence of a "catarrh of the bladder." And catarrh of the bladder is very generally regarded as a particularly obstinate, sometimes indeed as an incurable, affection. And I am free to confess that as long as these phenomena are considered referable to a specific disease, "catarrh," so long probably will the disease prove rebellious to treatment and sometimes even incurable.

Again, when the urine described is examined by the microscope, a quantity of pus, varying much in different cases, is seen to be present in it; and when examined by heat and nitric acid, a certain amount of albumen is, as a matter of course, deposited.

It happens to me, in the course of consultations, to observe that these phenomena—the admixture of pus with the urine and the presence of albumen—are, singly or together, frequently regarded in themselves, and apart from other facts, as necessarily presenting indications of very grave importance. Are they so?

Certainly they are by no means necessarily grave; on the contrary, in the great majority of cases of elderly men, the presence of these products is not grave. Let me demonstrate to you the marked distinction which exists between the significance which albumen possesses in two different categories of cases. I

can scarcely ask your attention to a matter of higher practical importance.

1. When a patient's urine, habitually clear, acid, and free from the faintest blood-tint, throws down to the test of heat and nitric acid, a notable quantity of albumen, the source of that albumen is the renal circulation, and, if persistent, the case is almost certainly one of grave import. The presence of organic change in the kidney structure is to be inferred, and other evidence of its existence, if sought for, will probably be found.

2. A very slight admixture of blood in any urine, no matter what the source of the hemorrhage, will produce a considerable deposit of albumen. It is evident, then, that the product in such cases, although sometimes grave, is not necessarily so, and that it may furnish an indication of the slightest possible import, inasmuch as a little blood may appear in the anterior passages, from a lesion which is slight and temporary in its nature.

3. Pus in the urine may, and most commonly does, proceed from some local condition of the bladder, occasionally, indeed, from local inflammation of the urethra. Nevertheless, albumen will be deposited on applying appropriate tests. It is evident that albumen, resulting simply from pus produced by chronic cystitis, has an import vastly less grave than that described above as No. 1, being a purely local and mostly temporary affection of an organ which has no vital function, but merely a mechanical one; the albumen in the former instance being evidence of disorganisation in the structure of a vital organ—that is, one the sufficient action of which is essential to the very existence of the body.

In short, there ought not to be the slightest temptation to confound two states so utterly unrelated as the two states which I have here contrasted, although they offer from one point of view an accidental similarity—that is, there is in both an admixture of albuminous material in the urine.

Still, nothing is more common than to hear, in connection with a case of purely local bladder affection, the remark gravely and significantly made, "I assure you I have on several occasions found by testing a large quantity of albumen in the patient's urine." It is a little difficult sometimes, although necessary, to listen to such an observation with quite sufficient patience. Does the observer really desire to intimate that the patient has constitutional albuminuria—*i.e.*, some form of Bright's disease? If not, his remark is simply devoid of meaning; since, as we know, there is vesical pus in the urine, we know equally that the albuminous constituent must appear on applying the test. And vesical pus in the urine certainly has

no more relation to constitutional albuminuria than pus which comes from an external abscess or surrounds a common boil. Simple as all this may appear to you and to me, it is quite astonishing how much confusion there is in men's minds in regard to this matter, and how much importance some persons attach to all albumen found in a urinary test-tube, although the source of the deposit may be easily demonstrated to be the bladder, and no other part of the organs which lie above it.

Finally, the important practical point in relation to treatment is first to ascertain the occasion of the local catarrh. In nine out of ten of these cases it consists in inability, often only to a slight extent, on the part of the patient to empty the bladder completely. The universally acknowledged cause, hypertrophy of the prostate, is, of course, the first in order of frequency. But after this are others not infrequent. Defective action may be due, first, to simple atony, the result of past habitual or occasional over-distension of the bladder with urine; secondly, to thickened and incompetent muscular parietes of the bladder after chronic inflammation, sometimes associated with old stricture; thirdly, to defective innervation seen in connection with other slight signs of impaired function in a nervous centre; the last being, of course, the most serious of all, in its nature and probable results.

In all of these local treatment, by carefully removing all the secretion by means of a soft catheter two or three times a day, perhaps aided by gently washing out some remainder, is the chief efficient remedy. Remember that this incompetence of the bladder is always to be sought for by physical examination; no other form of evidence in relation to it, as the patient's sensations, &c., is to be accepted as trustworthy. The introduction of a soft catheter immediately after the patient has passed water by his natural efforts, is the only test, and it should be applied on two or three occasions before arriving at a definite conclusion. The causal relation between the group of symptoms enumerated at the outset, and the defective function described, is far more common than it is generally supposed to be.—*Lancet*, *Jan.* 3, 1880, *p.* 6.

62.—ON RENAL CALCULUS.

By Thomas P. Pick, Esq., F.R.C.S., Surgeon to St. George's Hospital.

Calculi may originate either in the kidney or in the bladder, and they are formed in consequence of some peculiar state of the system, or diathesis, as it is generally called. And of these diatheses there are three principal forms, in addition to some other uncommon forms, of which it is not necessary at present

that I should say anything: the lithic-acid diathesis, the oxalic, and the phosphatic. Now it may be taken as a broad rule, to which, however, of course, there may be exceptions, that when a calculus originates in the kidney the nucleus is either of lithic-acid or oxalic-acid formation; whereas, on the other hand, if the stone is formed in the first instance in the bladder, the nucleus is either phosphatic or consists of some foreign body in the bladder, probably introduced into that viscus from without. All renal calculi may be said, therefore, to be constitutional and to arise from some morbid state of the urine, this state or condition depending on malassimilation. Now, when a stone forms in the pelvis of the kidney, it may either remain there, or it may pass down the ureter into the bladder. If it remains, one of several things will happen: either it will grow, extending into the ureters and calyces, gradually causing absorption of the substance of the kidney, producing excessive pain and irritation, and eventually death from exhaustion; or, on the other hand, it may grow, but without producing any irritation, and its presence may not even be suspected until the *post-mortem* examination, the patient in the meantime having died of some other disease. For instance, I show you this beautiful specimen from our museum, in which the kidney is one mass of stone. The "cortical and tubular portions have been completely absorbed, nothing of the organ remains, excepting the thickened external capsule and the projecting fibrous septa, which form a number of large sacculated cavities communicating with each other." And yet the presence of the stone was not suspected during life, and the patient was admitted into the hospital for quite another disease—typhoid fever, I believe, from which he died. So, again, here is another specimen in which the same thing has occurred. In other cases a stone lodged in the pelvis of the kidney may gradually mould itself to the form of the cavity in which it is contained, and, eventually blocking up the ureter, may produce suppression of urine. I show you here two kidneys, in which the orifices of the ureters are obstructed by calculi. They are not impacted, but lie in the funnel-shaped orifices, which they are polished to fit, so as to close them accurately. The preparation was taken from the body of a man forty-nine years of age, who died in the hospital, having for twelve days had incomplete suppression of urine. Lastly, a stone in the pelvis of the kidney may give rise to suppuration in and around that organ, the abscess may point externally in the lumbar region, and the stone be discharged through the opening thus formed.

Let us now consider the symptoms by which we should recognise the presence of a stone in the kidney:—First, there would be pain, and sometimes very great pain, in the loin, and

extending down to the hypogastric region and the genital organs. This pain would in some cases be much increased on moving about; so that—just as we find in stone in the bladder—the patient is comparatively easy so long as he remains perfectly quiet in the recumbent position; but if he moves about, and especially if he takes carriage or horse exercise, the pain becomes unbearable. I apprehend that the presence or absence of pain, and of this peculiar characteristic of it—its increase on movement,—depends in a great measure upon the size of the stone. If the stone is small, lying loosely in the pelvis of the kidney, it will be jolted about with the movement of the body, and thus, striking against the lining membrane of the cavity, it causes irritation, and so pain; if, on the other hand, the stone is large, it becomes more or less fixed, and, as we have seen, moulded to the space in which it is contained, and therefore it does not move with the various motions of the body, and the jarring or concussion, and hence the pain, is prevented.

Again, the nature of the stone will have something to do with the amount of pain. The rough, tubercular, mulberry calculus would necessarily cause more irritation than the smooth lithic-acid stone. Another symptom would be hematuria—blood in the water,—and this is of considerable importance. As you all know, bleeding may take place from any part of the urinary tract, but when it comes from the kidney it presents certain differences by which we can at once recognise its source. The blood is then intimately mixed with the urine, which presents the appearance of ordinary coffee, and in addition to this we often find blood-casts of the urinary tubules, which is, of all, the most certain sign, though, of course, less likely to be present in cases where the blood comes from the *pelvis* of the kidney.

Bleeding from the kidney may arise from—(1) Rupture or laceration; (2) congestion from nephritis or the use of certain drugs; (3) from a peculiar form of disease, which has been termed "intermittent hematuria"; (4) from cancer and other tumours; and (5) from calculi. Putting out of consideration the first three causes, in which our diagnosis would be sufficiently obvious, we must say a word or two upon the way in which we should determine whether, in a case of renal hematuria, the source of the blood was from the presence of a stone or from malignant disease in the organ. Without adverting to the ordinary points, such as the history of the patient, his constitutional diathesis, and his general appearance, all of which, nevertheless, would form important elements in our diagnosis, let me draw your attention to one particular circumstance, which I well remember used to be much insisted upon by the late Dr. Bence Jones when he was physician to this hospital. He used to teach that in bleeding from the kidney, when from malignant

disease, the blood would be greatest in quantity when the patient was warm and quiet in bed, whereas if the cause of the blood was a calculus, the bleeding would be greatest when the patient was up and about. When confined to bed the surface of the body is warm, and more blood is sent to the internal organs, and hence the greater quantity of blood in cases of cancerous tumour; whereas, as we have seen, in stone in the kidney, when the patient is moving about, it knocks or jolts against the wall of the cavity, and so by mechanical injury produces the hemorrhage. You will remark that in the case the notes of which I have read to you there was no blood in the urine, and in this way I think we may explain the fact; as I have just stated, the bleeding is produced by mechanical injury, and in this case, the stone, being large, was more or less moulded and fixed, and so this injury did not take place.

Another symptom which we should have in calculus of the kidney, and which would afford us an important aid in diagnosis, would be the presence of pus in the urine. As we have just seen, the disease with which it is most likely to be confounded is malignant disease of the kidney in its early stage. In fact, the late Dr. Basham, in his work on Diseases of the Kidney, states "that malignant disease can then hardly be distinguished, except, perhaps, by its antecedents, from calculus of those organs." Now, in the presence of pus in the urine we have an important means of diagnosis. In the early stage of malignant disease there is no pus. A patient afflicted with this affection will be liable to repeated hematurias, with intervals of rest, during which the urine becomes clear, apparently free from blood, and *contains no pus*. In calculous disease, on the other hand, even when the disease is in a quiescent stage, and the urine contains no blood, it will be found to be turbid and milky, and deposit an abundance of sediment, the purulent nature of which will be revealed by the microscope. I ought further to mention that Mr. Bryant insists much on the fact that the urine contains *pus*, and not *mucus*, as evidence that the morbid secretion comes from the kidney and not from the bladder.

We come now, however, to what appears to me to be the most important sign, and that is the possibility of being able to feel the stone. And there are two ways in which an attempt may be made to do this, either through the abdominal wall or by the rectum. Of course, the first, being the simpler measure, should be first tried, but failing to feel the stone in this way, I see no objection to introducing the hand into the rectum, and by this means reaching and exploring the pelvis of the kidney. I have no personal experience of this proceeding myself, but many of you may remember a patient in Fuller ward some twelve months or so ago, in whom Mr. Holmes introduced the

whole hand into the rectum without any difficulty. In Holden's "Landmarks" Mr. Walsham states that he has been able in some instances, by introducing his hand into the rectum, to reach the lower half of the left kidney. And he makes the remark: "Probably the existence of a calculus in the kidney might be detected." It is scarcely necessary to remark that this mode of investigation could only be employed when the disease was suspected to exist on the left side. In our patient it was not necessary to employ this means, because we were quite satisfied as to our diagnosis without; had we not been so I should not have hesitated to adopt it. The best means, however, because the simplest, is to endeavour to feel the stone, by palpation, through the abdominal wall. And if the patient is somewhat emaciated, as in all probability he will be, and the stone is of any considerable size, there is not much difficulty in detecting it. It is better to administer some anæsthetic in order to do away with the resistance of the abdominal muscles, and to empty the lower bowel by means of an enema. The patient then lying on his back, firm pressure is made on the front of the abdominal wall, and the stone, if it exists, will be felt in the loin, between the last rib and the crest of the ilium, about the level of the spine of the first lumber vertebra. It will seldom happen, I imagine, that we shall get such a certain means of diagnosis as we had in the case under consideration, where we had two stones in contact, and by moving them on one another produced a grating which could not be mistaken for anything else.

These, then, gentlemen, are the principal points by which you would be guided in arriving at your diagnosis in cases of calculus in the kidney. And now, having determined in your own mind that a stone exists, the question arises, what course are you to pursue? The medical treatment in these cases is unsatisfactory, and can at the best be only palliative. The only plan which holds out any hope of permanent cure is to remove the stone by operation. But are we justified in performing what must be regarded as a very formidable and serious operation for the relief of this affection? I think the answer to this question must depend upon the amount of irritation which is set up by the stone. We have already seen that in some cases the stone may remain perfectly quiescent for years, and without producing the slightest disturbance. If the presence of a stone under such circumstances were detected, I imagine no surgeon would advise its removal. Then, again, as Dr. Owen Rees has pointed out, stone in the kidney may after a time become encysted, and a patient gradually obtain relief of his symptoms. So that in cases where the symptoms are not very urgent, and have not existed for any length of time, it would, I imagine, be unwise

to operate, but would rather be desirable to wait in the hope that the calculus might become encysted. It seems to me that we should confine our operation to those cases where there is a marked and progressive deterioration of the general health. Where, in fact, the patient is becoming worn out by the constant pain and drain on his system from the hemorrhage or discharge of pus from his kidney.

The operation of nephrotomy for removal of a calculus from the pelvis of the kidney has not hitherto been attended by much success.—*Lancet, Oct.* 18, 1879, *p.* 566.

63.—ON A METHOD OF PERFORMING LITHOTOMY.

By FURNEAUX JORDAN, Esq., F.R.C.S., Professor of Surgery in Queen's College, and Surgeon to the Hospital, Birmingham.

The front of the bladder is its safest aspect; it is the aspect most sedulously avoided. We prefer to open the apex, which touches the peritoneum; or the base, which touches the rectum. Let us imagine what may seem at first sight a singular group of circumstances. A surgeon is about to cut for stone for the first time in the history of surgery. The patient is a man whose external genital organs and the triangular ligament itself have been swept away—say by an accident. The finger in the membranous and prostate urethra can draw the neck of the bladder well down towards the sacrum. The stone is large; in order that it may be removed, the bladder must be cut somewhere. The smallest, simplest, and safest incision will be one that enlarges the natural orifice in one direction or another. Now, is it conceivable that a surgeon unfettered by habit and tradition would think of cutting in any downward direction—of cutting towards the rectum or the delicate genital apparatus, and in a direction where safe space is most limited? But such a man, devoid of external genitals and of triangular ligament, is practically before us every time we operate for lithotomy through the perinæum. Such a man is *not* like a woman, in whom it may be objected that the upward incision is not generally accepted as the best. Quite otherwise. The urethra and prostate gland are deeper in the pelvis, are close upon the rectum, and, above all, are not held up firmly to the pubic arch by the triangular ligament.

The proposal, then, which I make is this: the membranous urethra being opened behind the triangular ligament, the deep incision—the incision in the prostate gland and the neck of the bladder—shall be made upwards and to an extent suited to the size of the stone. The extent, I believe, need rarely reach the capsule of the prostate. Where the firm fibrous submucous

tissue is divided, and the substance of the prostate scored, the dilatability of the parts is singularly great.

Touching the details: the first incision may be made as in the median or in the lateral method. The latter seems to me to give easier access to the apex of the prostate. I put a little angle in the outer wound, so that the concavity is towards the rectum, and all parts of it are more equally distant from the rectum. The membranous urethra should be freely opened. The well-oiled finger should then be gently carried along the staff through the uncut prostatic urethra, and brought into contact with the stone. The staff having been taken out, the finger firmly depresses the neck of the bladder downwards and backwards, while a straight blunt-ended bistoury is passed flatwise along its dorsum until the bladder is reached. The instrument, having its edge turned upwards, is carried by a quick movement a short distance into the bladder, and, with the edge still upwards, withdrawn in contact with the dorsum of the depressed finger. This proceeding gives a fairly uniform diameter to the wound. As a rule, pressure of the knife upwards is not needed; the urethra, being stretched on the finger, is incised to a sufficient degree by a single sawing movement. If the stone be known to be very large, free incision upwards is easily made, and in a direction which readily allows a large stone to be removed. The forceps is next slid along the surface of the still retained and still depressed finger, and the operation is concluded in the usual way, save that all manipulation by finger or instrument is directed downwards to the floor of the neck of the bladder and to the prostatic urethra. The staff in this operation should not be large (especially if the operator's finger be large). A staff large in the membranous urethra, and prolonged probe-like into the bladder, might be partially withdrawn while the finger was insinuated into the bladder; or a probe might be run into the bladder, as in the median operation, prior to the withdrawal of the staff.

A very simple expedient may be adopted where the urethra is small, which is especially useful in children. It is an expedient which some surgeons may prefer in all cases. A concealed bistoury, larger and stronger than the one used for dividing strictures at the meatus, may be easily carried into the bladder, after the membranous urethra is freely opened, along the staff (or even without the staff) in the urethra. A nervous operator may touch the stone with the instrument; the blade is then directed upwards and projected to a measured extent, and the instrument is withdrawn with the sheath pressing back towards the rectum. It seems to me that no lithotomy could be more simply contrived or formulated than this:

open the membranous urethra on a grooved staff; put a strong simple concealed bistoury into the bladder; project the blade upwards and draw out the bistoury; put the finger into the bladder, feel the stone, and extract as usual. Much may be done with this bistoury. It may be used as a sound; it may incise the prostatic urethra to any desired extent. After the incision (or before, if the urethra be large enough), the sheathed bistoury flatwise is an excellent guide for the finger to the bladder. All these advantages I experienced in the case of this little boy—my fourth case.

Whether the straight bistoury on the finger, or the concealed bistoury, be used, the instrument should be so held that the upward incision is confined to the prostatic urethra. The floor of the membranous urethra is already divided; its roof need not be cut.

I have found less than usual hemorrhage by this method; but, if it be at all free, say in adults, the application of a long sponge-tent for an hour or two effectually checks it. The tent may be withdrawn at will, without removing the tube.

I have been asked if there is no danger of urinary infiltration. Urinary infiltration is not a feature of the suprapubic operation, in which there is no downward drain, as there is here. I have had five suprapubic operations: three in little girls, one in a boy with traumatic stricture; all these recovered. One, a man with fatty viscera and with a stone that could not be removed through the perineum, died. In no case was there urinary infiltration. Here, in fact, the urine drains away from and under the deep cut, not over and into it, as in the lateral method. The upward incision in women, whatever other drawbacks it may have, is not attended with urinary infiltration.

The advantages of the operation seem to me obvious and numerous. The floor of the pathway to the bladder is uninjured, and underneath this floor lie the main dangers. The route to the bladder is more direct, less angular; and its diameter is more uniform. The operation is simple in principle, more definite in detail, more easily performed. In the lateral method, the far end of the deep cut is necessarily an uncertain point. If the knife stop short of the end of the staff, there is uncertainty; the position of the end of the staff itself is a variable one. The floor of the bladder is practically on a level with its neck, and therefore the vesical end of the deep incision is inevitably an unfixed and an unfixable spot. The front of the bladder, on the contrary, is at a sharp angle to the prostatic urethra, and furnishes a natural, definite, and measurable limit, to the vesical cut—a cut which begins at the angle and comes forward. The incision may be made larger at

will by definite and clear steps. Where the prostate is enlarged, it is a great gain not to leave large cut surfaces in its substance—surfaces which heal slowly or not at all. It is possible that an upward cicatrix might give unexpected relief in some cases of prostatic difficulty.

I have told you of my first case of several years ago. The next case was in a man near sixty, with enlarged prostate, but spare and temperate. Three years before, I had cut him by the lateral method, and removed a large stone. On the second occasion, I operated by the method I have described—treating the neck of the bladder somewhat after the fashion of the neck of the sac in strangulated hernia, and notching it upwards on the end of the finger. Three calculi, each averaging an inch and a half in diameter, were removed with great ease. The forceps were gently held along the whole tract, and not more at one spot than at another, a new experience to me. A long sponge-tent remained by the side of a tube for a few hours. His recovery was complete and rapid. The third case, which ended unfavourably, was that of a man aged sixty and extremely fat. He stoutly alleged that he lived a temperate life—falsely, as it was afterwards shown. The bladder was the deepest I have met with. Five calculi, all exceeding an inch in diameter, were removed. To my great regret, I found that the too zealous curiosity of some friends who were present had led them to insinuate their fingers upwards between the bladder and the pubes. This reminds me of the one great precaution to be taken: let there be no manipulation or examination in an upward direction. No purpose can be served thereby. After death on the third day, the heart and liver were found to be unusually fat. There was a limited patch of urinary infiltration above the pubes; it was not near the peritoneum, and there was no peritonitis. Probably it had nothing to do with the result.

My last case was in this little fellow eighteen months old. I opened the membranous urethra on a grooved staff, and then carried, without a hitch, a concealed bistoury into the bladder along the staff, but with no anxiety to keep in the groove. An instrument that passes readily in the direction of the bladder cannot go anywhere except into the bladder. Without caring to do so, I felt the stone with the end of the instrument. Turning the blade upwards and projecting it, I incised the neck of the bladder upwards. Keeping the closed instrument on the floor of the entrance to the bladder, my finger passed over it into the bladder with great facility. The forceps, introduced over the finger, seized and brought away two calculi in close contact with each other, and weighing together 185 grains. I never saw so little hemorrhage. He passed all

his urine by the natural channel on the seventh day. He is now in perfect health.

[Since the above left my hands, I have had a fifth case in a boy of five. He left the hospital well in a fortnight.]—*British Medical Journal, Jan.* 24, 1880, *p.* 115.

64.—ON LITHOTRITY, AND THE IMPORTANCE OF EARLY DETECTION OF THE STONE.

By GEORGE BUCHANAN, Esq., Professor of Clinical Surgery in the University of Glasgow.

The most recent conclusions arrived at may be best expressed in two propositions.

1. It is of the utmost importance that the presence of a calculus in the bladder should be ascertained before it has reached any considerable size; and as a corollary the bladder should always be explored with a sound, on the occurrence of the earliest symptom.

2. All small and moderate-sized calculi may be removed by the lithotrite, and, as a consequence, when this doctrine is accepted and carried into practice, lithotomy will gradually be abandoned, except in cases of an exceptional nature as regards the stone and patient.

First: the early detection of the stone. This is the key to the whole position. Of course it is true of all diseases that the sooner they are detected and their nature proved the better both for patient and surgeon. But it is true in a different sense for calculus, because it may make the difference between a nearly perfectly safe operation of lithotrity and a very dangerous one of lithotomy or lithotrity.

Let me recall for a moment the prominent symptoms.

(*a*) Pain in the perineum or the region of the base of the bladder, often accompanied by pain in the point of the penis. But pain is often altogether absent in early stages of calculus, and frequently present in cases of bladder disease in which there is no calculus; as in two cases under my care just now in which the patients describe the suffering at the orifice of the urethra as most acute.

(*b*) Frequent desire to pass urine. This is an accompaniment of irritation of the bladder from whatever cause, but being present, is a sufficient reason of itself to warrant a careful search for a stone.

(*c*) The presence of blood, either in the urine or coming in a few drops after urine has been passed. This also is present in many bladder and urethral affections, even in the second form mentioned.

(*d*) Interference with the continuity of the flow of urine—

i.e., either partial or total interruption of the stream, which recommences after a short interval or by change of position.

(e) Easily observable changes in the urine, as in colour, clearness or rather muddiness, thickness, viscidity, odour, &c., which are often remarked by the patient himself, and are the cause of his consulting his medical attendant without any of the other symptoms referred to. These are the evidences of chronic cystitis, which is more often idiopathic than the consequence of stone, but in many examples are the only symptoms to indicate the presence of a stone.

Now, with regard to these five symptoms which I have enumerated, I presume that if any patient came under medical treatment with them all present, there are few if any medical men who would not at once suggest that an exploration of the bladder should be made at the very first. But I am not so sure that it would be recognised as a general principle, that so soon as a patient presents any one or even two of them, the exploration should be made.

It is generally believed that stone in the bladder is not so common in this country as it was fifty years ago, and if that is true the necessity of searching for it is, numerically, not so imperative as formerly, still it is sufficiently common to make it important to have it in view in presence of the symptoms referred to. Since 1858, the date of my first lithotomy, I have had eighty cases of calculus in the bladder besides a few in the urethra.

Now I can assert that while in most of the cases which have come under my notice, two or more of the symptoms were present, yet in several instances only one of them attracted notice and continued the sole evidence for a considerable period, till the introduction of the sound proved the presence of a stone. And what I think ought to be recognised as a principle in all urinary disorders is, that as soon as one of the known symptoms of stone is discovered, the bladder should be explored by a sound. The manipulation scarcely ever gives any annoyance if performed with a properly constructed sound and a gentle hand, and the information obtained, positive or negative, is of the utmost importance. If the calculus is very small it may elude detection at a first examination, so that if the symptoms either continue or recur the sounding should be repeated. By this means a stone could be discovered before it has grown larger than a garden bean, and if the practice referred to were generally adopted, few stones would ever come to be dealt with of a size to be seen in all collections at the present day, except where from gross neglect on the part of the patient the symptoms were concealed and endured till increasing suffering drove the patient for advice.

I think the importance of searching for stone on the occur-

rence of *one* of the first four symptoms alluded to—that is, before there are any symptoms of cystitis, which is almost always accompanied by alkalinity of the urine—is rendered evident by a knowledge of the intimate structure of calculi; for an examination of any considerable collection of calculi shows that in the vast majority of examples, the nucleus of a calculus is uric acid or oxalate of lime, and that a phosphatic calculus is a very rare thing. When such a phosphatic stone does exist, it consists in almost every instance of an aggregation of smaller pieces held together by inspissated mucus, or which have become adherent to one another first by mutual pressure and then by consolidation of the intervening substance. Such calculi are somewhat porous, like pumice-stone. When in 1874 I removed my pathological collection to a room provided for me in the Western Infirmary, Dr. Foulis (at that time my house-surgeon) undertook to arrange and catalogue the preparations; and before arranging the calculi I got every one of them sawn through the centre, and thus exposed the internal as well as the external structure. And the examination of these sections convinced me of the almost universal prevalence of a nucleus of oxalates or urates, whatever the external shell of the stone might be.

Now, as oxalate and urate calculi are hard and presumably of slow growth compared with phosphatic, it follows that in the history of all calculi or nearly all, there is a time, and a considerable time, when they are small, hard, and of slow growth, and that is just the time when their presence may be indicated by the symptoms—pain, desire to micturate, blood, interference with the flow—occurring either singly or at considerable intervals, with intervening periods of total cessation of these symptoms, so as to throw the patient and even the medical adviser off their guard. And this is true to some degree even of the small number of calculi which are phosphatic from their first origin.

I think, then, we may conclude that if only the patient would apply on the first occurrence of anything unusual connected with his urine or the region of the bladder, and the medical attendant would have the bladder at once explored, calculi would be detected when of very small size, in almost all instances.

2. The second proposition I would bring forward is that all small and moderate-sized calculi may be removed with almost perfect certainty of a favourable result by lithotrity. This general statement must indeed be modified by the circumstances of each case, involving the condition of the patient and the nature of the stone. But I may state the case as follows: Given an adult in fair health, with a normal urethra, the blad-

der not too irritable, and containing a calculus not larger than a filbert,—any surgeon using the lithotrite with ordinary care may with confidence undertake to remove the stone by lithotrity with very little anxiety as to the result. But in the case of lithotomy it is different, as all practical surgeons know. There is anxiety as to the result even after an operation performed with dexterity and success, so far as the manipulation is concerned. Sir William Fergusson's words on this subject cannot be too thoughtfully considered—"There seems to me to be a mystery associated with lithotomy which has not yet been solved. For palpable errors there is an explanation; but when to all appearance there has been perfection in the operation, and yet death has been the issue, I confess I have been puzzled beyond measure." Again: "Working on inanimate material, no doubt precise manipulation must be of the most perfect effect; but when the phenomena of life are involved, the result seems in most instances beyond human control." I think every one who has had much experience of lithotomy will corroborate Sir William Fergusson's words.

I do not think that numerical results founded on the experience of any one in Scotland, as yet, will do much to help my argument; nor are the aggregate results as obtained from statistics of the Scottish hospitals sufficiently extensive to authorise any valid conclusion; still the experience of individual cases will often bias an operator in a way which is both intelligible and justifiable.

My first intimate connection with lithotrity was in the case of a gentleman who in 1861, when sixty-four years of age, was cut for stone by Mr. Syme, who extracted two uric-acid calculi. In 1864 a recurrence took place, so that in 1865 he had a stone removed by lithotrity by Sir Henry Thompson. In 1867 it again recurred, and I removed it by lithotrity in a fortnight, and from that time till his death in 1872 he continued perfectly free from all trouble in connection with urination. Now a comparison of the pain and discomfort which he suffered after lithotomy with the relatively little annoyance while undergoing lithotrity, made a most powerful impression on me in favour of the latter. Previous to this operation I had done my first lithotrity on a patient in the Royal Infirmary, in July 1866, with success. There are several gentlemen well up in years, from whom I have removed calculi by lithotrity with very little trouble or annoyance to them, now in perfect health; on whom, I confess, I would have performed lithotomy with very great anxiety and fear for the result. So that my own opinion is strongly in favour of lithotrity in favourable cases, and certainly always in the case of stones of small size.—*Lancet, April* 3, 1880, *p.* 520.

65.—ON BIGELOW'S OPERATION FOR STONE IN THE BLADDER.

By THOMAS SMITH, Esq., F.R.C.S., Surgeon to St. Bartholomew's Hospital.

The operation for stone to which we give the name of Bigelow's operation is a new one, founded on principles different from those which have hitherto guided us in the performance of lithotrity, and it is possible, or, rather, highly probable, that it will entirely reform our practice in the treatment of stone in adults.

And, first, I would have you understand what position the operation of lithotrity occupied as a curative measure before Prof. Bigelow introduced his proceeding. Lithotrity was used on adults, and for those stones which, if of an oxalate formation, did not exceed one inch in diameter, while in a general way it was pushed somewhat further in the case of those whose calculi were of a softer kind, either lithic acid, lithates, or phosphates; these stones might be crushed even if their longest diameter measured as much as two inches. Stones of larger dimensions than those I have mentioned were generally treated by lithotomy. Of course other circumstances than the age of the patient and the dimensions of the stone have to be taken into consideration in choosing between the two operations, but as these do not bear directly on the subject we have under consideration I shall not refer to them here.

The object in view in lithotrity has been at all times the removal of the stone with as little damage to the patient as possible, and in pursuit of this object certain rules have been laid down by professed lithotritists, and generally acted on by surgeons. Thus, in speaking of the operation and referring especially to the use of the lithotrite in a fresh case at the first sitting, Sir Henry Thompson says:—"The mere sojourn of an instrument in the bladder is a source of irritation precisely corresponding to the time, within certain limits, it continues there; anything, therefore, that will diminish the time of the operation and the amount of movement and concussion will necessarily give a greater prospect of success." And, again: "As a rule, then, at first commence with one introduction of the lithotrite, crushing four or five times during a sojourn of, say, two minutes in the bladder." As regards the after-treatment, Sir Henry Thompson tells us:—"You should not encourage the early passing of fragments. They rest at the bottom of the bladder, and I usually keep the patient in bed and pretty much on his back for thirty-six hours or so afterwards; he should, at all events for that period of time, pass urine in that position, so that the sharp angular fragments are left at the bottom of the bladder, and are not forced into the

urethra." The object of this delay in the passage of fragments being that they should, by remaining for a time in the bladder, become somewhat water-worn, and so less likely to irritate the urethra.

Mr. Cadge, who from his experience can speak with more authority than any other provincial surgeon, thus expresses himself on the same subject:—"In common with all English lithotritists, it has hitherto been my habit, at the first sitting, to do but little with the instrument, not knowing what the tolerance of the bladder may be, and at subsequent sittings to act more freely. I have of course met with cases, though they are rare, in which one sitting has sufficed to bring about a cure; more frequently there have been five or six, sometimes ten or twelve, and occasionally fifteen or twenty sittings, depending in a great measure on the size of the stone and the power of the patient to void the *débris*. Speaking generally, the chief principles of English lithotrity, as taught by Brodie and subsequent writers, are first to do as little as possible with the instruments, especially at the first sitting, leaving the extrusion of fragments as much as possible to natural efforts; and, secondly, to use small rather than large instruments."

You may gather from these statements that in the performance of lithotrity instrumental injury was more dreaded than any harm that might arise from the irritation of fragments of stone within the bladder, and that the removal of a moderate-sized stone from the bladder by lithotrity was a process prolonged generally over some days, and not rarely running into weeks.

It has recently been a growing habit with surgeons, since the use of anæsthetics in the operation, to remove after a crushing such fragments as could be washed away with the india-rubber apparatus called Clover's bottle, having been invented by the gentleman whose name it bears. Its object is to remove well-comminuted fragments after an ordinary lithotrity, where the lithotrite may have been used four or five times, and for this purpose it answers well enough; but it is not adapted for the rapid removal of comparatively large fragments of pulverised stone in considerable quantity.

Prof. Bigelow's operation is founded on different principles, and necessitates an entire change in practice. He maintains that the bladder is comparatively tolerant of the presence in its interior of smooth, polished instruments of a large size, even for a considerable time, but exceedingly intolerant of the rough and angular fragments of a broken stone. Thus the object of Prof. Bigelow is, if possible, to crush and remove stones at one sitting, believing that the advantage of complete evacuation of fragments is cheaply purchased by an operation

prolonged, it may be, for an hour or more. His operation, then, is planned for rapid and complete crushing and immediate evacuation of the fragments, and it has already been applied successfully to stones of far larger dimensions than a prudent lithotritist would have ventured to attack by the old operation. He employs a strong scoop-lithotrite, with a very wide female blade, so constructed that it can scarcely entangle the mucous membrane of the bladder within its blades if used with ordinary care, and having in the female blade an opening just beneath the spur of the male blade, which prevents the instrument from becoming clogged with the *débris* of the stone, and admits of a thorough pulverisation of the stone without withdrawal of the lithotrite; for you must know that an ordinary scoop-lithotrite, used on a moderately soft stone, soon becomes so choked with *débris* that the blades will not close, and the instrument has to be withdrawn. Bigelow's instrument is said to clear itself in its working, and not to need withdrawal on account of clogging.

For the rapid removal of fragments, Bigelow uses a series of large-sized catheters, running up to No. 20 English scale. These are straight, having an eye close to the extremity, well rounded off at its margins, and being exactly the size of the lumen of the catheter. The catheter is connected with an exhaust bottle by means of india-rubber tubing, and is removable at a bayonet joint. The india-rubber exhaust bottle is much stronger than Clover's, and has a glass trap for fragments attached to it below, also fitted with a bayonet joint. The bottle is worked in the same manner as Clover's; being compressed it distends the bladder with water, and on the removal of pressure its elastic recoil sucks out the fragments of stone.

Case of removal of four ounces of vesical calculi by Bigelow's operation, in two sittings.—J. B——, admitted on Nov. 25th, 1879, is a well-nourished man, sixty-nine years old, and has been for many years a sergeant in the police force. About twenty years ago he noticed that he did not pass his water so freely as he had been accustomed, but this did not cause him uneasiness. About nine years ago he was very much troubled by an uncomfortable pressing feeling at the neck of the bladder. This uncomfortable feeling continued till it became pain in the above situation. About twelve months ago the patient began to suffer from incontinence, which continued for a short time, and then ceased. Three or four months after, however, he again began to suffer with incontinence, which has increased to such an extent that he is obliged to wear a urinal. He complains that his water runs freely from him, and that he has no control over it. He suffers most incontinence

at night, and urinates best while lying on his right side. About five or six weeks ago he noticed that his urine was becoming turbid, and as it continued to become more muddy, and the feeling of pain at the neck of the bladder was increasing, he consulted a surgeon, who passed a sound, and thought he detected the presence of a stone. On the withdrawal of the sound the patient bled a little. He has resided for many years in Islington, and has been accustomed to walk about without inconvenience. He experiences no inconvenience when riding in an omnibus or in a railway carriage. He complains greatly of incontinence and a frequent desire to micturate. He is unable to pass water while standing. He tries to pass urine about three times in an hour. He passes it involuntarily while in bed. Urine, sp. gr. 1010, acid, no albumen, no phosphate; his urine is drawn off every morning and tested.—Nov. 26th: Mr. Smith passed a sound, and detected what he took to be phosphatic deposits.—28th: His urine is still acid, and contains much sediment. He was placed under gas and ether for twenty minutes, and Mr. Smith washed out his bladder with Bigelow's apparatus; about three ounces (dry weight) of stones were removed.—29th: The patient is very well, and feels no ill effects from the operation.—Dec. 4th: His urine is still acid, and contains much sediment; there is much incontinence. He usually passes about a pint and a half of water during the night. He was placed under gas and ether for about twenty-four minutes. Mr. Smith introduced a lithotrite and crushed some stones; he afterwards washed out the bladder; about an ounce (dry weight) was extracted. To facilitate the introduction of a large catheter the meatus of the urethra was enlarged by an incision downwards.—5th: The patient passed a good night, and feels no ill effects from the operation; is up and about the ward. Temperature normal.—8th: The patient is discharged. He is able to micturate while standing up, and no longer complains of incontinence. Urine acid; very little sediment. The stony matter consists of round small calculi and fragments, which appear to have been composed of similar stones. One of the calculi on section shows a round nucleus surrounded by concentric laminæ. On analysis, the concretion was found to be composed of uric acid in abundance, calcium, magnesium, ammonia (a trace), phosphates (a trace).

Here we have a man in his seventieth year, with more than four ounces of stones in his bladder, suffering from incontinence of urine, leaving the hospital four days after operation in comfort and cured, for the time at least, leaving not only his stone behind him, but his incontinence of urine.

Although I acted on Bigelow's principles, I did not use his catheters or his lithotrite; I could not procure his instrument,

so I used the lithotrite I thought I could employ most to the patient's advantage—a scoop-lithotrite, which was changed for a fresh one each time it became clogged by fragments. The catheter I used for the removal of the fragments was essentially Bigelow's, with a short curved beak to facilitate its introduction in my hands; its size was No. 16 English scale. The first sitting lasted in all twenty minutes, and during that time I was employed a quarter of an hour; in the second sitting, six days later, the patient was half an hour under the influence of the anæsthetic; the washing and crushing taking twenty-five minutes. He suffered no inconvenience of any kind from either operation, and the only blood seen was that which flowed from the incision of the orifice of the urethra at the second operation.

You may think that because on the first occasion I did not remove the whole mass of stones I did not follow out Bigelow's principles; but I would have you observe that the stones and fragments of stone were not of my making. His bladder had become to a great extent used to their presence, and I did not press the operation, as I judged that my proceeding, even if incomplete, would certainly diminish rather than increase the irritation, by diminishing the number of fragments in his bladder, and to that extent relieve his symptoms. As the patient was in the habit of passing small fragments of stone, I deemed it most prudent to remove first with the evacuating apparatus such fragments as would pass without the use of the lithotrite, and then to crush any stone or fragment that might remain behind.

The calculous matter removed by the operation weighed, when dry, more than four ounces, and consisted of globular stones of all sizes, from the size of a nut (these have been, of course, broken by the lithotrite) down to stones not larger than No. 5 shot. A countless number of these little stones have undergone spontaneous fracture in the bladder, their superficial layers splitting asunder and their nuclei escaping and looking like empty husks. This is a subject that has been studied by Dr. Ord, who attributes the spontaneous rupture of urinary stones to the central part of the stone being of different constitution from the external part; that the central part, containing mucus or other colloid capable of becoming swollen in an alkaline fluid, may burst the external laminæ on the urine changing from an acid to an alkaline condition. The stones in this case are chiefly composed of uric acid in combination with lime and magnesia.

We are not yet in a position to judge accurately of the value of Bigelow's operation as a curative measure; the materials for forming a sound conclusion do not yet exist, at least in a form

available for comparison. The largest recorded stone removed by the operation has been in one case of Prof. Bigelow, where the fragment weighed 1802 grains, and the greatest amount of calculous matter, consisting of whole and broken stones, removed by the operation, has been in the case of one patient on whom you saw me operate; here the fragments weighed 1920 grains.

Though the mortality among these cases has been low, it is probable that some instances where the operation has been less successful remain unpublished. And I would particularly direct your attention to the fact that it is not the mere death-rate of the operation that we want to ascertain. We must let a little time elapse, and, if possible, get some record of the condition these patients are in after the removal of their stones—whether their symptoms are removed, relieved, or aggravated; for you must know that, while lithtority can be frequently credited with real cures both of the stone and its symptoms, cases do occur in which the irritation set up by the lithotrite gives far more trouble to the patient than his original stone.

On the lines we have hitherto followed, the operation has achieved a fair success, I cannot say a brilliant success; but to whatever point we may have arrived in diminishing the mortality, the limit of progress has probably been reached on the old lines. If Bigelow's practice be founded on truer views of the whole subject (as it seems likely that it is), the domain of lithotrity has been greatly enlarged and a real advance will have been made in the treatment of stone in the bladder.—*Lancet, Jan.* 10, 1880, *p.* 43.

66.—ON LITHOTRITY AT A SINGLE SITTING.

By Sir Henry Thompson, Bart., F.R.C.S., &c.

Since I commenced to employ anæsthesia for lithotrity, I have gradually diminished the number of sittings, and removed more *débris* on each occasion. This progress was naturally in part the result of familiarity with the instruments and their use, acquired by many years of practice. I also employed more and more, during the last six or seven years, for the same purpose, and especially when the presence of irritation or inflammation made the removal of all the fragments particularly desirable, the aspirator of Clover. At the same time I have been in the habit of saying to younger men, who, as so many have done, accompanied me to my operations for the sake of studying the process, "Do not attempt on first beginning to do as much as you see that I venture myself to do." Such, at all events, was my practice until about two years ago; and in Paris the mode of operating then was, as I think, for the most part, it still is, based on the original "cautious" style, or many-sitting system.

About this time Mr. Bigelow, of Harvard, U.S., having tried the experiment in a few cases, proposed, as a general rule, that the stone should, if possible, be all crushed at one sitting, and removed entirely by a large aspirator and evacuating catheters, no matter how prolonged the time which might be necessary in order to accomplish the task. He believed that less damage would really accrue to the bladder and associated organs by a long, and even somewhat hazardous, sitting, provided the viscus was emptied of the *débris*, than by the old process of taking away a little and often, but leaving always hard and broken fragments in the cavity, to produce continued and sometimes serious irritation there during much of the period necessary for repeated sittings.

This was a bold, but I believe it was also a happy, idea. My mind was already prepared by past experience to receive it favourably, although the means Bigelow employed in the shape of instruments, especially the lithotrites he proposed to use for the purpose, it was impossible for me to approve; and I at once tried the plan, and have to a very great extent carried it out during the past twelve months. I am bound to say that, as far as I can judge, as far as now thirty-one consecutive cases thus managed enable me to determine, I think the principle a decided step of progress for lithotrity. Let the stone as to size be within the limit of the surgeon's powers, and let all be removed, or at all events the greater part of it, at the first trial. In attempting this, he should commence by crushing freely with a light but strong lithotrite, removing *débris* by the aspirator-current afterwards. This done, the lithotrite is again introduced, if necessary, to deal with the remainder, and is again followed by the aspirator. If the rattle of fragments in the bladder against the end of the evacuating catheter still exists after its use, the lithotrite is to be again introduced to break them down, and another employment of the aspirator will perhaps complete the task. If only a few small pieces still remain, it is better perhaps to leave them than to subject the parts to repeated manipulations in order to remove some tiny portion. Such will do no harm, and may be removed at a second sitting.

By this method, instead of limiting the sitting, as at one period was my practice, to a term of two or three minutes, it may extend from five or seven to twenty-five or thirty minutes, although a watchful caution must control the period as it becomes extended to the longer term named. The longest sitting I have as yet ventured on is twenty-five minutes, during which I removed 329 grains of a hard uric-acid stone from a gentleman of seventy-eight years of age. Three days after I removed 192 grains more in thirteen minutes, completing the case, and the result was quite successful; and when I last heard of him, he

was enjoying greatly improved health at the seaside. This and all the other cases referred to have been seen by many of my brethren both in town and country. It is worthy of remark that at present not one of the thirty-one cases has been fatal, but this number is insufficient to prove anything, as I once had an unbroken series of forty-eight cases by the old system without a death. All experienced operators know how large must be the numbers in order to furnish tabular results of a trustworthy nature. In reference to the time occupied in operating, I find I have hitherto removed on the average from 150 grains to 200 grains in ten minutes, of hard calculus (uric-acid or oxalate of lime). Of course, when the stone is friable, as in the case of phosphatic formations, a larger quantity may be removed in a given time.

For the purpose of this mode of operating, I have modified my instruments somewhat. I have used one lithotrite when necessary slightly stronger than before, making the male blade more wedge-shaped, or rather like the prow of a ship, in part angular, in part more bluff or obtuse; this male blade protrudes through a small opening at the bottom of the female blade, so as to make blocking up impossible. And the original aspirator of Clover, the principle of which is maintained in its entirety, I have considerably modified in a manner so that my new form of it offers three advantageous conditions: first, there is no possibility of air entering it; secondly, it ensures the shortest possible route into it from the bladder; and thirdly, it has a globular trap at the bottom, into which the fragments enter, but which they cannot leave.

The apparatus consists of a stout india-rubber bottle. On the upper part is a tap, and above it a small funnel through which to fill the bottle. At the lower end is a tube, and attached to it are the lower tap and the glass receiver. The tap is constructed thus:—Through the tube is fixed another metallic tube with a large opening on the under side, toward the receiver. The tube projects a short distance, to receive the end of the evacuating catheter placed in the bladder. The tap is contained in the interior of this tube, and is hollow; it is provided with a large opening in one of its sides, so that when once the tap is *in situ*, this opening faces that of the fixed tube containing it, and the end faces the opening, which fits the evacuating catheter. Next there is a spherical glass receiver; the metallic tube to which the receiver is fastened goes down into it about two-thirds of an inch. The spherical receiver is about two-and-a-quarter inches in diameter. When the operator compresses the india-rubber bottle the fragments enter and rise between the glass sides and the tube, and cannot mount again into the apparatus, in consequence of the projection of the tube into the

glass receiver. The apparatus acts thus:—The evacuating catheter being in the bladder, the tube is easily adapted. If the tap is now turned, communication is established between the bladder and the apparatus. If, on the contrary, the same tap is turned, all communication is closed, and the apparatus can be removed without spilling a drop of water. Nothing is easier than to fill the instrument. The upper tap is opened, the lower tap closed, and water poured in by the funnel at the top. The water fills it directly and drives the air completely out; the upper tap is then closed, and the instrument is ready for use.

The change in the situation and form of the lower tap shortens the distance many inches for the fragments to traverse, which thus reach the glass receiver by the shortest possible route. The current having less distance to travel, has a more powerful action on the fragments, consequently the aspiration is more perfectly made than it has hitherto been. The entry of air into the bladder is impossible, since the current leaves the lower part of the apparatus, and the air bubbles, if any, must rise to the upper part. In consequence, also, of its small size and portability, this instrument can be very easily adapted to an evacuating catheter, without necessitating any change in the position of the patient to be operated on. Lastly, for those who prefer a portion of flexible tube between the aspirator and the evacuating catheter, a portion is provided; two inches thus interposed, will ensure freedom from any jar to the bladder in using the instrument, and will only lengthen the route *pro tanto*. For myself, I prefer the absolutely direct and shortest route; having no more fear of hurting the bladder with the inflexible evacuating catheter than I have with the inflexible lithotrite.—*Lancet*, *Jan.* 17, 1880, *p.* 79.

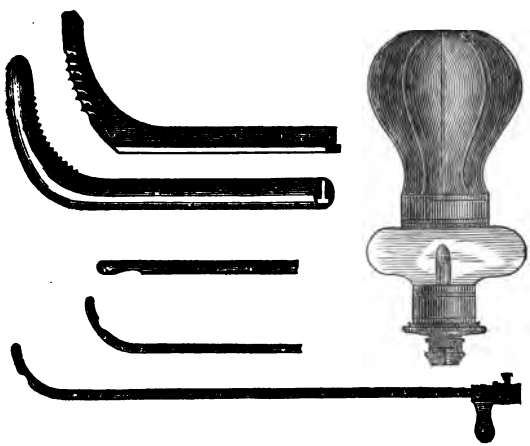
67.—A MODIFIED CLOVER'S BOTTLE FOR EXTRACTION OF THE *DEBRIS* AFTER LITHOTRITY.

By BERKELEY HILL, Esq., M.B., Professor of Clinical Surgery in University College, and Surgeon to the Hospital, &c.

Dr. Bigelow's bottle and tubes for exhausting *débris* after lithotrity are manifestly a great improvement on the apparatus previously employed for that purpose. The improvement consists, first, in providing a wider passage for the current to and fro between the bladder and the exhausting bottle; and, secondly, in furnishing a more forcible stream than was possible with the small bottle of Clover. But as Bigelow's bottle has considerable disadvantages in being costly, cumbersome, and especially in being much longer than is necessary, it occurred to me that Clover's bottle might, by some small

modifications, be rendered a more rapid exhauster of *débris* than that of Bigelow. For some time before Dr. Bigelow's visit to this country I had been employing urethral tubes for washing out *débris* larger than those commonly used for this purpose, habitually employing Nos. 24 and 26 French, instead of No. 22 or No. 23 (Nos. 12 and 13 English), the customary sizes. Consequently, when Dr. Bigelow brought before the profession his rapid method of clearing the bladder of *débris*, I was prepared to appreciate the improvement he has effected in this department of surgery. By the apparatus which I have now used on several occasions—the outcome of repeated alterations—I find I can exhaust a given quantity of *débris* more rapidly than I can by Bigelow's bottle. This advantage I attribute mainly to the shorter distance the fragments have to travel from the neck of the bladder before they are securely lodged in the trap below the current. The length of the tube from the beak to the interior of the receiver is only eleven inches, or twenty inches less than that in Bigelow's apparatus.

The woodcut shows the form of the apparatus very well. A stout india-rubber bottle, holding ten ounces, with a neck one



inch across internally, is fitted to a glass receiver, or trap, blown into a discoid shape resembling that of a flat turnip or onion, having a transverse diameter of 3¼ in. The mount of this receiver is closed by a cap, to which the washing out catheters can be fitted in a joint fastened by a bayonet-catch. The catheter is prolonged into the centre of the receiver by a short

tube, $\frac{6}{16}$ in. (about No. 40 French) in diameter, to prevent the reflux of the fragments that have been already sucked out of the bladder. As the fragments issue from the tube they at first swirl about in the receiver, then fall rapidly into the lower part, where they lie snug and still while the water is being injected again into the bladder to bring forth another quantity of *débris*. Probably the main advantage of the apparatus is its shortness. A moment's reflection will show that when the outflow ceases part of the *débris* sucked from the bladder lies in the tube, to be driven into the bladder again by the next inflow of water; consequently the nearer the trap or receiver can be placed to the neck of the bladder, the more rapid will be the removal of the fragments. In Bigelow's apparatus, the receiver is placed at the farthest extremity of the bottle. This large bottle is placed for convenience of working at the end of 14 in. of flexible tube. Thus the fragments have to travel 11¼ in. along the silver tube, then along 14½ in. of flexible tube, making a total distance of 26 in., before they reach the bottle itself, down which they gravitate during the pauses between each act of injecting and exhausting to the glass receiver placed at the further end of the bottle. Thus the fragments do not reach the trap or resting-place, until they have traversed 31 in. of distance. The tubes I use have an internal capacity equal to sounds Nos. 24, 26, and 30 French (equal to Nos. 14, 16, and 18 English). They are provided with large eyes, carefully bevelled off to prevent their gaping edges from lacerating the mucous membrane. Some have the eye placed at the convexity of the beak, some have it at the concavity, and one tube (the largest) is straight throughout. This I have seldom used, as it is rare for the urethra to admit its introduction without a preliminary free incision of the meatus urinarius, which it is not always expedient to do. Except in being shorter, and in the lesser sizes smaller, my tubes do not greatly differ from Bigelow's, and their mode of attachment to the washing-bottle is copied from Bigelow's pattern.

The woodcut also shows the beaks of a powerful lithotrite made for me by Mr. Coxeter, for breaking large hard stone that cannot be safely managed by ordinary lithotrites. It was constructed for me in imitation of Sir Henry Thompson's new lithotrite, and in some respect, like other imitations, advances beyond the first embodiment of the new idea. His recent alterations on former patterns Sir Henry Thompson has already brought before the profession in this country, and on the Continent. They are chiefly two. First, the male blade is fitted to slide along the female blade until its heel comes flush with the outer surface of the beak of the female blade, which is cut away for that purpose. By this arrangement fragments cannot lodge nor

lock the blades, but are driven through the female blade before the advancing male blade. The second novel point is, the knife edge form given to the beak of the male blade. This knife edge enables the male blade to penetrate a hard, bulky stone with facility, and split it into fragments, that can afterwards be readily comminuted by the usual lithotrites, which have more pulverising action. The beaks of the lithotrite depicted in the drawing differ from those of Sir Henry Thompson's instruments in having a somewhat sharper knife-edge and a wider notching of the edge; there is also a slight jagging of the opposing edges of the female blade to afford a better hold of the stone before it is broken. Whether this increased sharpness of the knife-edge is an additional advantage can only be determined by comparative trial of the two instruments. Mine was simply an endeavour on the part of Mr. Coxeter to produce a lithotrite for me that should embody Sir Henry Thompson's recent improvements, of the value of which there can be no doubt. Both Sir Henry Thompson's lithotrite and the repetition shown in the drawings, while being sufficiently powerful to deal with the hardest stones, are far less bulky and heavy than Bigelow's large lithotrite. This power is attained by better mechanical concentration of the crushing force.—*Lancet, Jan.* 24, 1880, *p.* 124.

68.—CASES OF STONE TREATED BY BIGELOW'S METHOD.

By WALTER J. COULSON, Esq., F.R.C.S.

Case 1.—Francis A., a clerk, aged fifty-four. Oct. 2, 1879. About three years ago he passed three small calculi, and a year and a half afterwards symptoms of irritation of the bladder began to reappear, but did not become very urgent until quite recently. He now complained of frequency of micturition, pain in the region of the bladder and urethra, and occasional incontinence of urine, which contained a large quantity of mucus. The patient's general health was tolerably good. A calculus was readily detected on sounding. Oct. 13. The patient being placed under the influence of ether, I crushed the stone about eleven times, using several lithotrites in succession. Bigelow's exhausting apparatus was then used, and by its means a great quantity of detritus was removed from the bladder. The instrument had to be withdrawn, as a fragment had become impacted in its distal orifice. After reintroduction a further quantity of *débris* was removed. The straight tube of Bigelow's apparatus was found to answer better than the curved one. After the operation the patient expressed himself as very comfortable. There was no trace of rigor; he slept well, and had to pass urine only three times during the night, though on first falling asleep a little escaped involuntarily. On the following

day the patient was able to hold his urine for three hours, and complained only of a little smarting along the urethra during micturition. The urine was tinged slightly brown, but was moderately clear, and contained some fine grit. Oct. 16. Urine much clearer, but still contains excess of mucus. The patient can retain his urine for more than three hours; no incontinence, and no grit passed. 22. I examined the patient with the litbotrite and sound in succession, but could not detect any fragments. The man can now hold his urine for four or five hours; there is neither pain nor straining in micturition, and no incontinence at night. The urine still contains a slight excess of mucus, to check the formation of which four ounces of decoction of triticum repens was ordered three times daily. The weight of the fragments removed during the operation was 130 grains.

Case 2.—Thomas B., aged seventy-five, artist. Oct. 13, 1879. He had always enjoyed good health, and never had gout or any serious illness. Some acute lumbar pains, which he had thirty years ago, would appear to have been of rheumatic origin. Three years ago he passed some reddish gravel, but none has appeared since. His present symptoms have troubled him for about a year. They are mainly great frequency of micturition, especially during the day, pain in the perineum during and after micturition, occasional retention of urine, but never any hematuria. The patient is a thin, healthy-looking man, not appearing as old as he is, and tolerably active. Florid complexion; no sign of arterial degeneration. Passes urine four or five times in an hour when moving about, but not more than once as a rule during the night. The urine is of a light amber colour, and acid reaction; sp. gr. 1018. No abnormal constituents beyond a little excess of mucus. There is no family history of stone or gout. Oct. 15. The patient having been placed under the influence of ether, I introduced a lithotrite, and discovered that the bladder contained two calculi. One of these, about three-quarters of an inch in diameter, was first seized and then crushed eleven times in succession with one instrument and nine times with a second. The second stone was then sought for, and crushed repeatedly, several lithotrites being used in succession. A large quantity of detritus was removed at each withdrawal of the instrument, and some difficulty was caused by portions of *débris* getting loose in the urethra. Before introducing the tube of Bigelow's apparatus it was found necessary to enlarge the meatus, owing to the smallness of that aperture. Both the straight and curved tubes were used, some considerable difficulty being experienced in introducing either tube through the prostatic portion of the urethra. Eventually the receiver of the apparatus was attached to the tube and the

bag gently squeezed. Slight rotation of the straight tube caused it to pass onwards into the bladder. A considerable quantity of finely powdered detritus was then drawn into the receiver. The patient was under the influence of the anæsthetic for forty-two minutes. After the operation there was neither rigor nor discomfort except a little smarting, due to the enlargement of the meatus. The urine passed after the operation was blood-stained, and contained fine dust. Total quantity of detritus removed 195 grains. Temperature same evening 100·4°; pulse 120. On the second and fourth days after the operation there was a rigor, followed by sweating, but the symptoms passed off without any ill effects. On the 22nd a lithotrite was introduced, and several small fragments crushed, and the operation was repeated a week later, when Bigelow's apparatus was again used, and a few fragments evacuated. After this date all the urinary trouble abated, and on the 7th Nov. the patient declared himself to be quite well. Nothing abnormal could be detected in the bladder.

Case 3.—Robert C., aged sixty-eight, Nov. 25, 1879. Had suffered from symptoms of calculus for about eighteen months, but his sufferings had become much aggravated during the last four months. He complained of severe pain in the back and frequent calls to pass urine, especially during the night. The pain was much aggravated by exercise. No history of gout or rheumatism, and none of his family appear to have suffered from stone. The patient's expression was anxious, his tongue very dry, white and furred; great thirst; slept badly; pulse 114; temperature normal. Urine abundant, very high coloured, with a fetid ammoniacal odour; reaction alkaline; albuminous (about one-tenth); deposits triple phosphates in abundance; no casts; sp. gr. 1015. On examination with the lithotrite several calculi were detected; a rigor followed. The patient was kept quiet for a few days, during which the constitutional symptoms somewhat improved. The pulse fell to 90, and the nights were less disturbed. On Dec. 3, the patient being placed under the influence of ether, I crushed several stones, using the fenestrated and plain lithotrites in succession. Bigelow's curved tube was then introduced, and a quantity of very fetid urine escaped. A large quantity of detritus removed by means of the exhausting apparatus. The tube was then withdrawn, and the lithotrite again used, after which more detritus was removed. The patient was under the influence of the anæsthetic for fifty minutes, and bore the operation well. On examining the detritus several small calculi intact were discovered; these had escaped the lithotrites. The total amount of *débris* was three drachms. At the conclusion of the operation a few calculi and fragments appeared to be still in the

bladder. All the symptoms were relieved by the operation. The patient passed urine freely during the succeeding twenty-four hours, when it was thought expedient to introduce a catheter to remove any residual urine. About a pint escaped, muco-purulent and very offensive in character. The catheter was afterwards used every four hours, and the bladder washed out night and morning with dilute Condy's fluid. This gave much relief to the patient, and improved the character of the urine. A week after the operation, however, the urine was still muco-purulent, distinctly albuminous, and had an offensive ammoniacal odour. Dec. 17. The character of the urine and the general symptoms having improved, the operation was repeated under ether as before. After several crushings with a fenestrated lithotrite, 270 grains of *débris* were evacuated. After the subsequent use of a plain instrument 110 grains were removed. The patient was under the influence of ether for forty-two minutes, and the operation was well borne. In the after-treatment the urine was withdrawn as before, and diluted Condy's fluid used to wash out the bladder. On the 21st the patient was reported as sleeping well, with pulse and temperature normal, and the urine comparatively clear and free from offensive odour. The quantity passed, also, which had been excessive, sometimes reaching as much as six pints in the twenty-four hours, was normal. On the 22nd I detected and crushed a small fragment, which was all that remained in the bladder. The patient was allowed to be up daily, the urine being still drawn off every four hours.

Remarks.—The three cases above detailed afford convincing evidence of the tolerance which the bladder exhibits of prolonged instrumentation, and of the advantages of Dr. Bigelow's modification of lithotrity. In the first case, in which the patient's symptoms were comparatively mild and of recent standing, the relief was immediate, and there was no drawback in the form of a rigor or local disturbance. On the tenth day after the operation the patient was well. The second case was one of greater severity, and there were two calculi to be dealt with. It was not, however, till the second day after the operation that any constitutional symptoms appeared, and these were comparatively trifling in degree, and soon abated. The difficulty experienced in introducing the evacuating tube was due to the fact that the lining membrane of the urethra, especially of the prostatic portion and near the meatus, had become coated by pulverulent *débris*, which obstructed the passage of the instrument. This difficulty was at once removed by injecting fluid through the tube passed down to the prostate, thus washing out the neck of the bladder. Under similar circumstances, Dr. Bigelow used the female blade of a small urethral lithotrite

in order to remove the abundant and closely adherent calculous matter from the walls of the urethra. The condition of the third patient was in many respects unfavourable for any operative procedure. There was considerable constitutional disturbance, while the pain, frequent micturition, the state of the urine and its unusual quantity, pointed to severe vesical and renal irritation. The result of the operation, however, proved that the proper course had been adopted. All the symptoms were forthwith ameliorated, and although the operation lasted fifty minutes, and involved protracted manipulation, it was followed neither by local nor constitutional troubles of any kind. The second operation was equally successful. The fenestrated lithotrite was found to answer better than the plain instrument. The former does not pulverise the calculus, but reduces it to small fragments which are not repeatedly caught by the instrument, but are sufficiently reduced in size to pass readily through the tube. Besides this the fenestrated lithotrite is readily freed from *débris* by simply screwing the male blade home, and thus the repeated withdrawals and reintroductions, necessary in the case of the plain instrument, are dispensed with. The escape of *débris* takes place more readily through Bigelow's straight tube than through the curved one. The difference was demonstrated by the quantity which escaped (110 grs.) in the second operation on the patient, after only three crushings with the plain instrument. The case altogether well illustrates the advantages of Bigelow's method. The total amount of detritus removed at the two operations was far greater than could have been got rid of in any reasonable time by the unaided efforts of the patient, while the freedom from secondary, local, and constitutional troubles, and the direct amelioration of all the symptoms, may fairly be attributed to the rapidity with which the bladder was freed from the source of irritation—an object which Bigelow's method satisfactorily accomplishes. The adoption of the method has the further advantage of enlarging the range within which lithotrity is applicable, and it encourages the surgeon to deal with much larger stones than was formerly the case. What the limits of the operation, as determined by the size of the calculus, really are, further experience alone can decide, but the prospect of greatly diminishing the number of cases subjected to lithotomy on account of the dimensions of the calculus—a class in which the mortality is especially great—seems almost certain to be realised.—*Lancet, Jan.* 31, 1880, *p.* 161.

69.—LITHOTRITY AT A SINGLE SITTING—CASE.

By Prof. WILLIAM STOKES, F.R.C.S., &c., Dublin.

The following case is, I think, worthy of record, being one in

which I performed the operation of lithotrity first in the Aug. of last year, when I removed after four sittings a phosphatic calculus weighing 86 grs., and again in July of the present year, when I removed a calculus weighing 56 grs. by a different method, viz., the one to which Prof. Bigelow has given the name of "Litholapaxy." The case was of a man æt. 60, who was first admitted into the Richmond Surgical Hospital on August 19, 1878. For two years he had suffered acutely from all the ordinary symptoms of stone, which are unnecessary here to recapitulate. On sounding the patient the stone was at once struck. On Aug. 22 I performed lithotrity in the ordinary manner, and repeated the operation on Aug. 29, again on Sep. 9, and lastly on Sep. 19—four crushings in all. After these operations no trace of calculus could be detected on careful examination being made by my colleagues and myself, and the patient expressed himself as relieved from all vesical trouble. After the second and fourth crushings very grave constitutional disturbances were observed, especially after the last; so much so as to give rise to serious apprehensions on my part as to the result. However, eventually the patient recovered and returned home, free from all the pain and vesical irritation he had so long suffered from. Immunity from the trouble, however, was not destined to be of long duration. In about six months after the patient left the hospital, the pain and frequent desire to micturate again commenced, and on his readmission into hospital four months subsequently, a calculus was again found to exist. As there appeared to be no contra-indication to the adoption of Bigelow's method of operating, I determined to adopt it in this case, especially as I had a vivid recollection of the alarming symptoms the patient had after the former lithotrity operations; I determined therefore, if possible, to remove the stone at one sitting. On July 17, assisted by my colleagues—Drs. Thomson and Corley—I operated, using the straight evacuating catheter No. 18. This was introduced with facility after the orifice of the urethra was enlarged by incision. The operation lasted an hour and ten minutes, and a calculus weighing 56 grains—and which I have now the opportunity of exhibiting to the Society—was removed. Thompson's full-sized lithotrite was employed, and introduced three times during the operation. A half-grain morphia suppository was then administered, and he soon after fell asleep. At seven o'clock p.m. the patient had a violent rigor, and both pulse and temperature rose. Diffusible stimulants and opium were then given, and no recurrence of these symptoms was subsequently observed. On the fourth day after the operation severe abdominal pain was complained of, which, however, completely yielded to the exhibition of opium. There was no recurrence of it. On the

ninth day after the operation the patient expressed himself as relieved from all vesical trouble, and on the twelfth day left the hospital at his own request. I have seen the patient twice since he left the hospital, and on both occasions he expressed himself as completely free from the distress he had so long suffered from. This is, so far as I am aware, the first time the operation of Bigelow has been performed in Ireland.—*Medical Press and Circular, Jan.* 7, 1880, *p.* 1.

70.—THE TREATMENT OF STRICTURE OF THE URETHRA, WITH SPECIAL REFERENCE TO RECENT PROGRESS.

By W. F. TEEVAN, Esq., B.A., F.R.C.S., Surgeon to the West London and St. Peter's Hospitals.

Dr. Otis has shown us that we have greatly under-estimated the calibre of the urethra, and what good results we shall obtain if we raise our standard in the treatment. Let us not forget that without Otis there probably would have been no Bigelow. Dr. Otis demonstrated what large instruments could be passed down the urethra, and Dr. Bigelow utilised the discovery for his new method of lithotrity.

All strictures range themselves into two great divisions, the *passable* and the *impassable*. The former usually are as easy of treatment as the latter are difficult. I do not think that this great difference has been sufficiently insisted on in their classification, and it would seem to be the tendency of all surgeons to occupy themselves more or less with the passable form of the complaint. The impassable stricture is always situated low down in the urethra, unless produced by traumatic causes. Fortunately for surgeons, although deep strictures are the most difficult to enter, they are the most amenable to dilatation. I will commence with the treatment of the passable variety.

There is, firstly, what may be called the "expectant" plan, which consists in confining a patient to bed and trusting to warmth, rest, and purging for an abatement of the trouble. This is not a desirable method, and has but few advocates. It entails a great sacrifice on the patient, and any amelioration effected rapidly vanishes when he resumes his ordinary methods of life.

Secondly, there is the treatment by gradual dilatation with instruments, allowing the patient to follow his occupation. There is a general consensus of opinion amongst surgeons all over the world that this is the best method of treating ordinary dilatable uncomplicated stricture. It is attended with no risk to life, and involves no sacrifice on the part of the patient. Certain American surgeons object to it, alleging that the treatment is merely palliative. Inasmuch, however, as I consider

no more can be claimed for any other method, I hold that their objection is not founded on good grounds. With what kind of instruments ought the process to be carried out? We have soft and metal bougies, and each have their respective advocates, although I am glad to say the partisans of the cold steel are gradually diminishing in numbers every year, to the infinite benefit of mankind at large. If anyone has a doubt as to which is the better instrument to use, a soft or a metal one, let him one day pass a soft one on himself, and a few days afterwards introduce a metal one. We need not wait for an answer. Who has ever seen a man die after the passage of a soft French catheter? Are not cases on record in which patients have lost their lives from the mere introduction of a metal catheter, sound, or lithotrite? Who has ever seen a case of abscess and perineal fistula from the passage of an elastic olivary bougie? I have myself come across patients in whom abscess and fistula have followed the introduction of metal instruments by eminent surgeons. Are not rigors often seen after passing metal catheters, and are they not almost unknown after the use of soft ones? On every ground, therefore, let us use soft instruments. "La sonde rigide ne cede pas, l'obstacle cede devant elle." (Mercier.) But I have heard surgeons say, "Why, my patients actually prefer a metal bougie to a soft one." No doubt they do when the soft one is badly made. A viciously-shaped soft bougie is more annoying to the patient than a metal one. The latter may be steered into the bladder, but the former cannot. There are bougies and bougies. English bougies are badly adapted for the treatment of stricture, for, being cylindrical, they are blunt at the end, and cause the maximum amount of friction. The only use to which they can be put is to replace the dangerous wire stylets of metal catheters. In the French olivary bougie we have an instrument which gives rise to only a minimum amount of friction, and is admirably adapted for the purpose. It was invented by Lioutt at the commencement of the present century, and was formed by placing an olive on the end of a conical bougie. Its tapering flexible extremity enables it to adapt itself to the windings of the urethra, and the olive tip prevents it from lodging in the lacunæ or false passages. A well-made French olivary bougie has a thin long neck and a well-developed head, whereas the so-called French bougie made in England has usually a thick neck and badly-shaped olive, which, as Van Buren has pointed out, renders the instrument useless for treatment.

Most cases of passable stricture can be gradually and efficiently dilated with olivary bougies; but every now and then when there is a long tunnel stricture, the soft conical bougie

will be the best to deal with it. Either of the instruments can be strengthened by inserting stylets of lead, as seen in the "bougie-à-plomb" of the French. Bougies filled with small shot, as used in Paris, glide into the bladder almost by their own weight. Although I am a strong advocate for soft instruments, I recognise that metal ones have their uses. In connection with the subject I would remark that the English gauge ought to be abolished. Its transitions in size are abrupt and meaningless. If a No. 10 Eng. is passed one day, and a No. 12 Eng. the next, what does it signify? Nothing beyond the fact that a larger instrument has been introduced, but how much larger cannot be said. Gradual dilatation, when carried out by the English guage, often fails on account of the abrupt transition in the size of the bougies used. In the French guage the transitions are very gradual, and are mathematically exact. If No. 18 be passed, and after it No. 19, it is known that one millimetre in circumference has been gained.

Thirdly, there is the method of continuous dilatation, which is most useful when the former plan has failed, or where a patient cannot afford the length of time requisite for gradual dilatation. It, however, sometimes fails on account of the irritation set up by the retention of an instrument, and occasionally it is impotent to effect the desired object. Mercier has shown that perforation of the bladder may be caused by tying in a catheter, and he points out that a cylindrical instrument, having its eye close to the end, ought to be used, the eye being made to correspond with the meatus internus, so that the catheter does not project into the bladder. Mr. Furneaux Jordan and I have shown that tight strictures can be well treated simply by tying in a filiform bougie; the urine runs by its side and dilates the stricture.

There is, fourthly, the method of sliding one instrument over the other, chiefly known here as "Wakley's." It is very useful in certain cases where there are false passages; for when once the guide has been passed tubes increasing in size can easily be slid over it.

There is, fifthly, cauterisation. At one time it was extensively used in this country, but it has been almost completely abandoned. The experiments of Reybard demonstrated that it was a most objectionable method on account of the character of the cicatrix which resulted. Now, although I do not think it eligible for passable strictures, still I have seen it useful in cases of impassable stricture, where the opening was placed very eccentrically; and it was employed not for the purpose of enlarging the stricture by causing ulceration, but simply to open up the mouth of the contraction, to enable a bougie to be introduced.

Sixthly, electrolysis. Mallez and Tripier were the first to employ it in the treatment of stricture. They were followed by Newman, of New York; all of them, however, merely using it to cause absorption. Dr. Van Buren says it has been tried and found wanting. It appears to me to possess one great disadvantage, and that is the long period of four weeks which must elapse between each introduction. I had some time ago a patient under my care who had a perineal abscess and fistula caused by its application in America.

There is, seventhly, the method of over-distending the stricture by introducing an instrument with two blades, which can be expanded. It is very painful on application, and the results are of a most ephemeral nature. Fortunately, it has only a few advocates.

The operative treatment of stricture.—Under what circumstances is an operation justifiable? I consider no operation called for until it can be proved that the manipulative treatment has been tried and found wanting. I am indeed unable to conceive what conditions can justify us in exposing a patient to the risk of an operation, however slight, unless we have previously given other means a fair trial. But there are circumstances which may demand an operation: (1) the stricture may be non-dilatable, and resist all means to enlarge it; (2) it may, on the contrary, rapidly dilate, but as rapidly contract again, so that the patient has so frequently to pass a bougie to keep the passage open that the complaint becomes an intolerable nuisance to him; (3) there are some men of peculiar idiosyncrasy, especially those who have resided in the tropics, in whom the introduction of any instrument is followed by urethral fever, which greatly prostrates them, and interrupts the treatment; (4) a patient may be ordered abroad, and have only a short time to devote to his treatment, long enough, however, for the performance of an operation, but not for a course of gradual dilatation. I consider that in all the above instances an operation is justifiable.

All operations on passable strictures may be arranged under one or other of the following heads:—1. *Incision*, which includes internal, external, and subcutaneous urethrotomy, and also scarification. 2. *Laceration*, which embraces the so-called immediate treatment, splitting, rupture, divulsion, and over-distension when carried beyond the stage of true dilatation.

Before proceeding to a selection, the ground will be cleared, and an answer rendered easier, by asking what it is we really require when about to undertake an operation for the relief of a passable stricture. What we want is to enlarge the calibre of the urethra where narrowed, so that it shall be equal to, or, at all events, not much less than that of, the healthy portion. When a surgeon cuts a stricture, he can choose what place he likes for

operation, and he knows the length, depth, and extent of his incision; whereas the operator who tears open a contraction cannot select the spot, and is entirely ignorant of the length, depth, and extent of his laceration. If only a portion of the circumference of the urethra be invaded by the contractile deposit, the healthy portion will be torn, leaving the strong indurated part untouched. But it may be said, how can a surgeon find out whether the roof, side, or floor of the urethra be affected? By using the *bougie-à-demiboule*, which will tell him whether the deposit involves the entire circumference of the canal, or only a portion. The most important fact which should influence us in selecting a cutting operation rather than a lacerating one is that the mortality attending the so-called immediate treatment is much greater than that which attaches to internal urethrotomy.

Internal urethrotomy remains as the stock operation for stricture. There were several objections to the urethrotome as invented by Maisonneuve, some of which were removed by modifications effected by Voillemier and Sédillot. The most serious one, which was that the instrument when introduced gave no evidence of where it had gone to, still remained. This difficulty was overcome by me. I utilised the slot which extended from end to end in the instrument by filling it with a closely fitting stylet, so as to prevent its being closed by blood or mucus. By this arrangement the surgeon can absolutely determine whether the urethrotome be in the bladder or not, for urine will escape when the stylet is withdrawn if the instrument be not in a false passage. I also considerably curtailed the length of the slot, which was unnecessarily long, and thereby shortened the possible path the knife could travel, thus rendering the instrument safer to use by inexperienced hands. Sédillot placed the sheath *inside* the staff of the urethrotome, thereby necessitating a considerable increase in its thickness, which was objectionable. I put the sheath and its stem *outside* the staff, and thus greatly strengthened the latter without increasing its bulk. If the stricture be very tight or crooked, a slender olivary bougie is wriggled through it into the bladder. A staff, filled with a stylet, is then screwed on to the bougie and made to follow it: the latter coiling up in the bladder. The stylet is now withdrawn, when urine will escape by flowing out of the open slot where it emerges from the penis. The knife, covered by the sheath, is then placed inside the slot, the latter being outside the staff. The sheath not only protects the healthy urethra, but it also represents the finger in the urethra, and tells the surgeon where to begin to cut and where to leave off. The urethrotome, as modified by me, meets, I believe, all requirements.

Some years ago I wrote an article in the Lancet advocating the division of strictures from before backwards in preference to the opposite direction. Mr. Berkeley Hill, in his critique on my article, stated that the principle of division of a stricture from before backwards, which I advocated, "had been proved over and over again to be quite unsatisfactory." I am glad to say that Mr. Hill has now come round to my way of thinking, and practises what he formerly condemned. At a period not very remote the principle of dividing strictures from behind forwards was the one in vogue at the special department for urinary diseases at the Necker Hospital, at University College Hospital, and at St. Peter's Hospital. Now all is changed, and at each of those institutions strictures are incised from before backwards. Ought the contracted urethra to be divided at the roof, side, or floor? The roof is the safest spot for incision, as the bulb is thereby avoided, and no troublesome bleeding occurs. Then. again, as the upper wall of the urethra is not so long as the lower one, a shorter cut suffices if it be made in the roof rather than the floor. If, however, the deposit is very thick and mainly affect the floor, it is better to divide it there, which can be done by simply reversing the urethrotome when in the bladder just as one does a lithotrite.

Until recently it was the practice for all surgeons in every country to leave in a catheter after every form of urethrotomy, the operator being threatened with "disastrous results" if he omitted to do so. It was reserved for Gouley of New York to to prove, clinically, that the retention of a catheter after operation favoured the very occurrences it was supposed to prevent. I have seen perineal abscess caused by retaining an instrument, but I never observed any such annoyance where it was not used.

After every form of urethrotomy I commence to pass a large metal "bougie olivaire à ventre" about the fifth day from the operation, and repeat the introduction every third day. Metal instruments are indicated, as they tend to straighten the urethra, often crooked before operation.

External urethrotomy is an admirable operation for those exceptional cases of stricture complicated by the presence of abscesses or numerous fistulæ or massive chronic induration. The operation was popularised in America by Jameson at Philadelphia, and later on by Syme in Great Britain. At one period it was extensively practised, but it is now very properly reserved for exceptional cases. There were two serious objections to the operation as performed on a "Syme's staff." 1. There was no evidence that the staff when apparently introduced into the bladder was really there. Hence humiliating accidents occurred to surgeons through the instrument having gone into a false passage which was mistaken for the urethra.

2. Although the operator knew where to commence to cut, he did not know where to cease, so it often happened that when the stricture had apparently been divided, it was impossible to pass the catheter, owing to some fibres having escaped cutting. The latter objection was removed by Mr. John Marshall, who employed a slender, solid metal staff, over which he slid a flexible catheter which could be moved up and down at will, and passed over the staff into the bladder if the stricture were thoroughly divided. The former objection was done away with by me. With Syme's or Marshall's staff there was no certainty as to the instrument being in the bladder, or not. I replaced the catheter by a catheter staff, which when introduced into the bladder permits the urine to escape, and so demonstrates its position. The catheter has an olive point and a slit in the convexity, along which the knife can be run to divide the stricture. When the instrument is passed into the bladder, the stylet is withdrawn, and urine escapes. A long slender staff is then screwed on to the catheter, and the elastic tube passed over it till arrested by the stricture. By means of a screw at the end of the tube the latter is fixed in its position and the movable handle attached. When the surgeon thinks he has divided the stricture, all he has to do is to advance the elastic tube, when it will pass into the bladder if all constricting bands are divided.

Subcutaneous urethrotomy was apparently suggested by Syme, but put into practical operation by Dr. Henry Dick. It is admirably adapted for the division of those very thick ring-like strictures often met with in the penile urethra. The operation, however, requires more skill for its performance than either internal or external urethrotomy.

The treatment of impassable stricture is one of the most difficult and important subjects which can engage a surgeon's attention, and demands from him much skill, care, and patience. Professor Syme laid it down as a rule that if urine could escape a catheter could be got through the stricture. His own practice, however, afforded evidence that his dictum was not absolute. The difficulty often depends not so much on the extreme tightness of the stricture, as on the deviation of the canal, causing the entrance to be placed very excentrically. Consequently filiform bougies are invaluable for the reduction of impassable stricture. If only one of them can be introduced, a fine silver catheter open at both ends can be slid over it into the bladder, or a fine silver catheter may be screwed on to the "bougie conductrice," and made to follow it into that viscus. Olivary whalebone bougies, with coudée or corkscrew ends, are most useful for tough tight strictures. Catgut bougies are of no value, for when they have been in the urethra for a few

minutes they become soddened and unravel. Before attempting to introduce any instrument the canal ought to be well injected with oil, as recommended by Perrève. A patient with an impassable stricture should be kept to bed and well purged, for rest, warmth, and castor oil are invaluable in converting an impassable contraction into a passable one. Confinement to bed very often makes all the difference between failure and success.

One of the most useful manœuvres to effect an entry is that of Mercier's. A straight metal tube, open at each end, and as large as the urethra will admit, is passed down the canal till it is arrested by the stricture, against which it is kept firmly pressed so as to render the face of the contraction as tight as a drum. An olivary whalebone bougie, with coudée end, is then introduced, and the face of the stricture methodically explored till the entrance is found. If this does not succeed, bougie after bougie is to be passed till the tube is filled. An attempt is then made with each bougie separately to see if it can be wriggled through. I have in many cases employed the "bougie armée" successfully, opened up the mouth of the stricture, slipped a bougie into the bladder, and ultimately cured the patient.

If retention from impassable stricture should set in, it is well to remember that it is not necessary, as I have proved, to introduce a catheter to relieve the stoppage. This fact is important, for as the smallest bougie is more slender than the smallest catheter, the former may pass where the latter cannot. The ultimate cause of retention in tight strictures is in all probability owing to the blocking up of the narrowed canal with a plug of inspissated mucus; hence, if this be pushed back by a bougie, urine will flow. The practice of filling the rectum with ice, whilst the surgeon is attempting to introduce an instrument, was introduced by Cazenave of Bordeaux, and I can myself speak very highly of the practice.

If an operation be required, which shall we select? Tapping the bladder through the rectum is a favourite one in this country, founded on the theory that if the urine be diverted from its ordinary channel for a few weeks the congestion and irritation will subside and the stricture become permeable. It is, however, a very disagreeable procedure, as it necessitates the patient's confinement to bed with a tube up his rectum; and if the operation should fail in its intention, a second one becomes necessary. Then there is an operation invented by Dittel, which consists in passing a catheter, through a puncture made above the pubes, into the bladder through the prostatic urethra till it meets the obstruction, against the front of which a catheter has been passed down the penile urethra. The canal is then opened

on the point of the latter instrument, and a dissection made till the point of the other catheter is struck. I consider this operation unnecessarily severe; all that it accomplishes can be achieved by Mr. Furneaux Jordan's operation, without the special risks incidental to Dittel's procedure. Inasmuch as the prostate is not the seat of stricture, the urethra can be opened in its membranous portion from the rectum, as practised by Mr. Jordan. Then there is the method known as Cock's operation, which I consider ought only to be performed for retention or for those cases of impassable stricture where the patient is too weak to undergo a prolonged operation, which the Boutonnière sometimes is. I hold that the Boutonnière is the right and proper operation to be performed for all cases of impassable stricture, for the constriction is opened at its front and divided, thereby remedying cause and effect, if retention be added to the impassable condition. It is no doubt the most difficult in all surgery, but it has been greatly simplified through the labours of Avery, Wheelhouse, Gouley, and myself. The operation, as performed by Wheelhouse, consisted in opening the urethra half an inch above the stricture, and insinuating a probe through it into the bladder. Gouley substituted an olive-tipped bougie for the probe; and as the stricture is often very tortuous it is clear the flexible bougie may pass where the inflexible metal probe cannot. Gouley uses the bougie as a guide to cut along, and slips his tunnelled catheter over it into the bladder. The great defect in the operation, as performed by Wheelhouse and Gouley, is that the position of the probe or bougie is not proved before cutting; hence either may have gone into a false passage instead of the true channel, without the operator being made aware of his mistake. Now I prove the position of the bougie by sliding over it a fine metal catheter, open at both ends, and having a slit in its convexity, along which the tenotome can be pushed. If the bougie be in the bladder urine will escape from the catheter when the former is withdrawn. I then divide the stricture *subcutaneously*, and to ascertain that this is completely effected I slide over the silver tube, a large elastic one, four inches long, which will slip into the bladder if the road is clear. By the means I have related the surgeon is spared the mortification of mistaking a false passage for the urethra. Mastin of Mobile, after dividing the stricture subcutaneously, passes a catheter which he ties in, and then brings the edges of the wound together over the instrument in order to obtain union by the first intention; thus treating the case like rupture of the perineum.—*Lancet, Jan.* 24, *and Feb.* 28, 1880, *pp.* 121, 318.

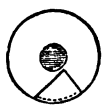
AFFECTIONS OF THE EYE.

71.—ON CORNEAL TRANSPLANTATION.

By Dr. J. R. WOLFE, F.R.C.S.E., Lecturer on Ophthalmic Medicine and Surgery in Anderson's College, Glasgow.

In the spring of 1872, I communicated to my excellent and learned friend, Mr. Henry Power, the following case, which was then under my care. The patient had met with an accident to his eye while working in a shipbuilding yard. I found the lens opaque, and bulging forward along with the iris into the anterior chamber, so that it almost touched the cornea; the small space left was filled with bloody fluid, which obscured the extent of the wound, and allowed a very fine tear of the cornea to escape observation. I proceeded at once to extract the lens by an inferior linear section, which was very easily effected; but after the lens came away, I found that the injury to the cornea formed two sides of a triangle, which was almost completed by the corneal incision (Fig. 1). We were thus left with a loose triangular piece of cornea. As the eye looked in every way healthy, and the operation was so far satisfactory, I simply replaced the loose portion of cornea, and kept the eye shut for two or three days longer than usual in such cases. When I opened it, I found that vision was perfectly restored, and, to my great astonishment, the loose piece of cornea had not only adhered, but had preserved its transparency. I mentioned this as a remarkable fact, as it pointed in the direction of replacing an opaque cornea.

Fig. 1.



Shortly afterwards, Mr. Power exhibited some cases of transplantation of the cornea from the rabbit to the human subject, by which, although it did not lead to any practical result, he rendered great service to science in familiarising us with the subject. He has since published details of a case where he transplanted a human cornea, which he had preserved in a solution of chloride of sodium.

I satisfied myself with the transplantation of conjunctiva from the rabbit to the human subject for the cure of symblepharon, and succeeded in restoring vision (by artificial pupil) and mobility to many useless eyes. This method I have the satisfaction of finding generally adopted as the only remedy applicable to such cases.

With regard to corneal transplantation, after a series of experiments extending over several years, I have reached certain conclusions, which may be stated as follows:—

1. The cornea can maintain its vitality and transparency when transplanted from one place to another, but to be successful it must be taken from a freshly enucleated human eye.

2. All the incisions in the cornea must be clean, as any tearing is likely to lead to suppuration, and the measurements of the graft, &c., must be exact.

3. It must be done in such a manner as not to damage the subjacent structures. This is absolutely impossible if we attempt the transplantation of the whole of the cornea, because as soon as we remove it the lens and vitreous come out, bleeding follows, and loss of vision is inevitable even if we secure corneal transparency. But the removal of the whole cornea is not necessary, and the chances of maintaining its vitality decrease in proportion to its extent. Hence, if we aim at the restoration of sight by transplanting a transparent cornea, we must content ourselves with a small corneal graft, removed along with conjunctival bands, which not only assist to keep it in position, but also by their adhesion afford it a chance of preserving its vitality, even if we do not get immediate union between the corneal edges.

These being the axioms which I have established in connection with the subject, my next aim was to devise instruments which would answer all the conditions.

In March 1877, Dr. Sellerbeck, of Berlin, visited our institution, and to him I explained the principles of my proposed operation, complaining of the inefficiency of all the instruments which Paris and London makers had supplied to me for the purpose. Dr. Sellerbeck got an instrument made for him in Berlin, and performed one operation which was successful in restoring sight. This case he exhibited to eminent surgeons in Berlin.

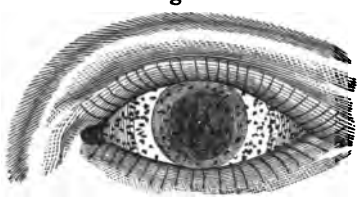
I do not mention this with the view of asserting my priority of discovery, for indeed, in a paper which he published, he shows that corneal transplantation has long been the yearning of German surgeons; besides, I consider all disputes about priority puerile, and would never trouble any journal in vindicating my claim. If another gentleman has succeeded in showing the first successful case, he is welcome to the credit of it. My operation for corneal transplantation, and for detachment of the retina, were suggested to me, not by the reading of old records, but by observation of the process followed by nature in the repair of injuries.

Having at last succeeded, as I think, in finding out an operative method calculated to fulfil the conditions neccessary to a successful issue, I have operated, on Oct. 29, upon an almost hopeless case of opaque cornea, the appearance of which is shown in Fig. 2.

John P., about forty years of age, met with a serious injury two years ago by an explosion, which burned his hands, face, and head, and almost destroyed his eyes. The right eye is

completely disorganised, and the conjunctival sac partly obliterated; in the left (shown in the figure) the conjunctiva is studded with powder and *débris*, the margin of the cornea is covered with a false membrane, the rest of it is totally opaque, with a perforation about the middle, where the iris protrudes. The eyeball is decidedly soft (T. 2). When examined with a lighted candle he unhesitatingly follows the flame in every direction, and this retinal sensibility to light is the only point of encouragement to attempt to restore sight by corneal transplantation; in other words to risk a good cornea upon it.

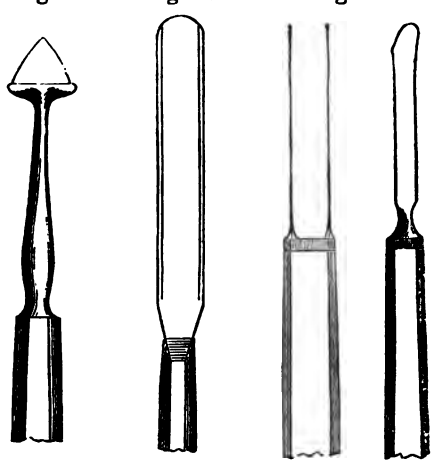
Fig. 2.



Indeed, this cornea was intended for another patient, whose case presented much more favourable conditions, but through some mistake he was not forward in time, and as our present patient was the only one at hand, I proceeded to operate on him—I may say, with considerable reluctance.

The instruments I use are—a lance with a stop (Fig. 3), a

Fig. 3. Fig. 4. Fig. 5.



grooved director (Fig. 4), and a double-bladed knife (Fig. 5) which fits into the grooves of the director. These vary in size according to requirements, and each set is marked with a num-

ber corresponding to the breadth of the graft in millimetres. Thus, my first set is marked 5, and is adapted for a flap five millimetres in breadth.

The patient being put under chloroform, a flap, broad at the base and converging to the cornea (see Fig. 7), is taken from the ocular conjunctiva on each side, and dissected up to the transition fold. These are turned over on the cornea, and the eye removed in the usual way. The lance is then introduced in such a way as not to injure the conjunctival flap, and pushed in as far as the stop will allow. The same is done on the opposite side. The director is then passed through one of the openings, and pushed in front of the iris and lens, out through the opposite one (Fig. 6); the knives are placed in the grooves, and the corneal flap separated. This is put into a little tepid water.

The other patient is then put under chloroform, and a similar corneal and conjunctival flap removed; but in this case the lance must be used in such a way as not to interfere with the pillars of the iris, and it must be rapidly withdrawn, so as to prevent as much as possible the escape of the aqueous humour, and the consequent falling forward of the iris. The transparent graft is then placed in position, and secured by stitches in the corners of the conjunctival flaps (Fig. 7).

In our case we found that the staphyloma above referred to, and other iris adhesions, were such that no broad director could be used. I had therefore to proceed by introducing my small probe-pointed corneal knife, turning the sharp edge up, and cutting out the cornea on one side; then, steadying it with forceps, I had to separate the other border with a single sweep of long sharp scissors. Thus we could not secure that straight symmetrical lacuna desired, and had to shape our flap somewhat tapering towards the nasal side so as to correspond. The dressing applied was simply three strips of adhesive plaster, dry lint, and a bandage.

Fig. 6.

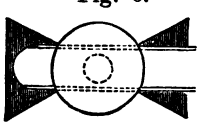
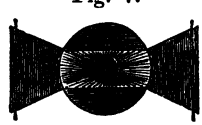


Fig. 7.



Under the circumstances we could hardly have expected a satisfactory result; but when the eye was examined, on the fifth day after the operation, we found the corneal and conjunctival grafts adhering like sticking-plaster. From the time of the operation until the eye was opened not a tear came from it, and there was not the least inconvenience or discomfort. We

may say, immediate union had taken place (this I ascribe greatly to the conjunctival bands). The patient could distinguish features and count fingers. The improvement has been going on steadily, and on the ninth day, when examined by the Presidents of the Faculty of Physicians and Surgeons of Glasgow, and the Philosophical Society, he could not only count fingers, but could see to such an extent as to put his finger upon a ring on the hand of the examiner. The Professor of Physiology in the University saw him on the fourteenth day, and satisfied himself that the man had the power of exteriorating quickly and certainly.

I think it interesting to bring this case before the notice of the profession, not only for the remarkable issue of a case so hopeless that hardly any benefit could have been anticipated, but for the sake of pointing out the solution of the problem, how to transplant a human cornea safely and satisfactorily.

The principle of corneal transplantation seems to me to be sufficiently established, and it is only the *modus operandi* which has been occupying my attention. Now having, as I think, found a method of rendering this operation feasible, I would submit it to the profession for trial. Its important practical bearing is evident, considering the large number of persons who owe their blindness solely to an opaque cornea, and the many enucleated eyes, with transparent corneæ, which might be rendered available to supply the deficiency.—*Medical Times and Gazette, Nov.* 22, 1879, *p.* 579.

72.—HOW TO OBVIATE THE RISKS OF FAILURE IN OPERATIONS FOR CATARACT.

By Dr. J. R. WOLFE, F.R.C.S.E., Surgeon to the Glasgow Ophthalmic Institution, &c.

In former years—from 1745 till 1860—the extraction of an opaque lens from the eye was effected by a method which was regarded as the classic operation for cataract. It was introduced by Daviel, and consisted in making a semi-circular incision in the cornea at its margin, and removing the lens through the pupil.

Of all the new procedures, none has attained such a position as that introduced by von Graefe in 1865, under the name of "modified linear extraction," in which he substituted a small linear incision for the flap.

In cases of infantile cataract, I very rarely leave the lens to be got rid of by absorption, considering that a good many eyes are lost through the irritation caused by the lens substance acting on the iris and other structures. Generally, after opening the capsule widely, and leaving the lens to be softened in

the aqueous humour, I remove it three or four days afterwards by puncture with a broad needle, which is introduced obliquely through the corneal substance, so that upon its withdrawal complete coaptation is preserved, rendering hernia iridis impossible. Having been an opponent of the introduction of pumping instruments into the cornea for soft cataract operations when first recommended to the profession, I am gratified to see that several surgeons of eminence, who, led by high authority, then advocated that system, have, after experiencing disasters, come to my way of thinking. A soft or softish lens can easily be got rid of, expeditiously and safely, by means of the broad needle, aided by the fixation forceps at the vertical meridian of the eye on the opposite side. But when the lens is of a firmer consistence, the aspiration will drag upon the vitreous and other delicate structures, and is sure to do mischief without making an impression upon the lens substance.

In cases of senile cataract, I make an iridectomy generally downwards, from two to three weeks prior to extraction. In a case of urgency there need only be an interval of a few days between the iridectomy and extraction, as the two incisions do not interfere with each other. The section of the iris is made subconjunctivally, with a lance of the size shown in Fig. 1. Should the iris not prolapse on the withdrawal of the instru-

Fig. 1 shows the size of the lance.
Fig. 2, the corneal knife used to divide the bridge.

ment, it is drawn out with Tyrrell's hook, and its internal circular border is laid hold of with a small pair of iris forceps, and rounded off with scissors, in such a way as to imitate the natural pupil and leave the ciliary border untouched.

In the operation for extraction, the patient being in a recumbent position, I introduce a speculum and fix the eyeball by means of forceps tipped with gutta-percha, and with Graefe's knife I enter the external margin of the cornea close to its sclerotic junction; the counter-puncture is made directly opposite on the inner side, the points being so chosen that the corneal flap extends a millimetre more than the third of its circumference. The knife is then pushed in a plane parallel to the iris. When the corneal section is nearly completed I withdraw the knife. In cases of soft vitreous I open the capsule widely with the point of the knife before withdrawing it. I then remove the speculum. (2) With my thumb and index-finger serving the

purpose of a speculum, I introduce the cystotome, and make sure that the capsule is properly opened; and in cases of soft vitreous, when the lens has a tendency to sink, I make gentle pressure upon the eyeball, and, using the cystotome as a crochet, I bring the lens forward and put it in a position facing the corneal wound. (3) I introduce a small blunt-pointed corneal knife with a convex edge (Fig. 2), which I direct obliquely to divide the corneal bridge. (4) After a few seconds of rest, I seize a fold of the upper eyelid between my fingers and depress the lower with my other thumb, and, directing the patient to look straight forward, I exercise pressure upon the lower part exactly in the vertical meridian facing the middle of the iris section, and, with the other fingers through the eyelid, I press upon the wound to make it gape, when the cataract advances to the corneal incision. Any *débris* remaining behind is easily extruded by gentle manipulation. (Fig. 3.)

A B C D

Fig. 3 shows the position of the incision, and appearance of the pupil, in Daviel's, Graefe's, and the author's methods—A, in Daviel's; B, in Graefe's; C and D, in the author's. In C the dotted lines shows the subjunctival iridectomy puncture.

When the vitreous is soft I leave a larger corneal bridge, and instead of finishing the circle at the corneal margin, I carry it more forward into the corneal substance, so as to protect the vitreous from prolapsing. I avoid giving chloroform, and do not dilate the pupil previous to extraction, as then there is a risk of a part of the iris falling upon the knife.

The advantages I claim for this method of procedure are the following:—

1. The iridectomy is not looked upon as a separate operation, but merely a slight preparation, as the patient is only confined to bed for the afternoon.

2. We can shape the iris section with precision, so as to secure a nice appearance, which can only occasionally be accomplished after the cornea has been incised for the removal of the lens. (See Figs. 4 and 5.)

3. We secure the same acuity of vision as when the pupil is intact.

4. The edges are rounded off in such a manner as to prevent them being invaginated in the wound, and giving rise to sympathetic ophthalmia, which has been laid to the charge of Graefe's operation.

5. We escape having the process of extraction obscured by the bleeding, which is inevitable when the iridectomy immediately precedes the extraction.

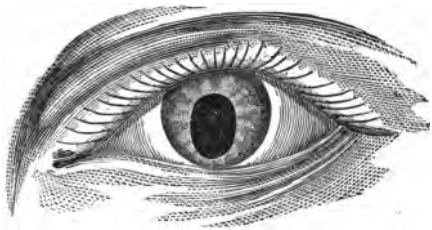


Fig. 4. Appearance of an eye after the extraction of a lens, done last year, when the extent of the iridectomy was still considerable.

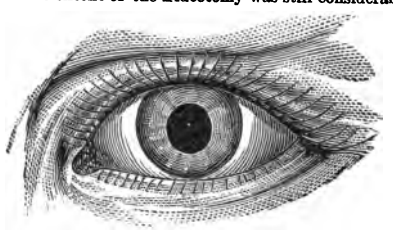


Fig. 5. Appearance of the pupil in an operation done just recently, after we commenced to restrict the extent of the iris section.

These are not abstract speculations, but really dangers to be guarded against. Many a time when witnessing operations performed by surgeons who occupy a high position in our art, I have seen, after iridectomy, the anterior chamber filled with blood which could not be got rid of, and the lens extracted in the dark, part of it coming away, the remainder making a mass with the coagulated blood. At other times again, when pressure was made upon the eyeball with the curette, the nucleus was squeezed out, but a mass of cortical substance remained behind, which would not yield to pressure; the issue of such cases could hardly be doubted.

In cases of traumatic cataract my procedure depends entirely upon the nature of the injury. When the lens is *in situ*, I make an iridectomy at some distance from the corneal laceration, if there be any, and extract the lens at a subsequent period. When the lens has been dislocated forwards, I remove it without interfering with the integrity of the pupil, unless the iris has been hurt, when I take the precaution to remove the injured portion. When the lens is dislocated backwards into the vitreous humour, and is opaque, the patient being seated

on a chair and the head inclined forwards, I introduce a needle through the sclerotic, and bring the lens forward into the anterior chamber. I then entrust the needle to an assistant, and remove the lens through a small corneal incision, without interfering with the iris.

Without entering into statistics, which might be the subject of cavilling, I may state generally that the results of the operations performed in the Glasgow Ophthalmic Institution, in presence of numerous students and medical practitioners, who see the cases daily afterwards, have been highly satisfactory; and though they include cases of unripe cataract, fluid vitreous, and other local and constitutional complications, the loss of an eye is such a rare occurrence that, unless in a combination of untoward complications, we never calculate upon its possibility.

In venturing to recommend the advantages of this operation to the profession, I need hardly say that the operative procedure must be adapted to the varying conditions of each case; but the leading principles here enunciated, when strictly followed out, I consider a safeguard against the risks which render cataract operations precarious.—*Lancet, Dec.* 6, 1879, *p.* 827.

73.—ON EXTRACTION OF CATARACT BY SUCTION.

By T. Pridgin Teale, Esq., M.A., F.R.C.S., Surgeon to the Leeds General Infirmary.

[Dr. Wolfe, of Glasgow, having, in a paper published in the Lancet (see preceding article), condemned the operation of extraction of soft or semi-fluid cataracts by suction, Mr. Teale replies:]

As the introducer of this method into British ophthalmic surgery, I feel called upon to vindicate its claims to the confidence of the profession; and having used the method for nearly twenty years in perhaps three-fourths of my cases of soft cataract, I may fairly lay claim to an opinion of some weight against that of one who was "an opponent of the operation when first recommended," and "is gratified that several surgeons of eminence," having had disasters, "have come to" his "way of thinking."

Let me summarise my ideas and matured views in the following propositions:—

1. When a cataract is almost fluid it will readily flow out in the groove of the ordinary curette, and there is no need of extraction by suction.

2. When a cataract is immature or semi-solid, to attempt to withdraw it by suction is to put enormous atmospheric pressure upon the ocular structures and to risk disaster.

3. For cataracts between these extremes—and they constitute

a large proportion of soft cataracts—there is no method, to my mind, more simple, more speedy, more effectual, more safe, or less liable to be followed by opacity of the capsule, than the method of suction.

4. To use a suction instrument with safety requires attention to minute details, no less than is needed for the use of the lithotrite. Lithotrity carelessly performed becomes disastrous. Performed with due care and gentleness, it is one of the most successful of the major operations of surgery.

5. The safest form of suction instrument is the one in which the action is regulated by the mouth. The "pump syringes" are more difficult to use, and do not admit of an equal delicacy in the regulation of the suction power, and, therefore, demand most exquisite manipulation in the operator.

6. The aperture in the curette ought to be oval, and level with the upper surface of the curette, so that on the completion of the flow of cataract the cornea may close the opening and act as a valve, arresting further and mischievous suction. It is this valve-like action of the cornea which is one great safeguard in the use of suction, a safeguard which is impossible in the case of the instruments made some years ago, in which the opening was enlarged by notches at the side.

7. The suction force ought to be applied with gentleness, with *short draws* of the mouth, and with no great force. If the cataract proves to be stiff, and requires great suction power, the attempt to extract by suction should be abandoned, and further proceedings should be deferred until the wound is soundly healed.

8. On no account whatever ought transparent lens to be withdrawn by suction. Firstly, because it is too firm to be safely drawn into the curette; secondly, because, owing to its transparency, it is impossible to judge whether the whole lens has been removed or not.

9. "When the lens is of firmer consistence, and the aspirator drags upon the vitreous and other delicate structures, and is sure to do mischief" (Dr. Wolfe), to use suction is surely to misuse it. It is hardly fair to condemn the use of a method because abuse of it produces disaster.

10. It is of supreme importance to preserve the posterior capsule of the lens unruptured, otherwise the vitreous humour comes forward, and is drawn into the curette instead of the lens.

In conclusion, may I venture to refer to my own experience in support of the views herein expressed?

As far as I can recollect, I have had but three cases of destructive inflammation after the use of suction. In the first case, a syphilitic boy, diffuse keratitis set in, first in the eye

operated upon; afterwards in the untouched eye. The other two cases occurred about six years ago, so closely upon each other that I suspect that some such cause as suggested by Mr. Carter may be the explanation—viz., that the curette conveyed decomposing material into the eye.

On referring to the notes of my private operations for soft cataract (the record of my hospital cases not being complete enough for comparison), I find that of my last fifty-three cases of soft cataract, in forty-seven suction has been used. Of these only one has been lost by destructive inflammation. Of the forty-seven, seventeen are noted as reading No. 1 or 2 Jäger. Of the remaining thirty, many are noted as "good sight"; many were too young to have their sight tested; and two or three, though perfect as operations, had defective retinæ. These facts, though very imperfect as statistics, show—1. That I am still as convinced of the value of suction in the treatment of soft cataract as when I first brought it out. 2. That it is possible to use suction extensively without "experiencing disasters."—*Lancet, Jan.* 3, 1880, *p.* 29.

74.—THE SUCTION TUBE IN CATARACT.

By R. Brudenell Carter, Esq.

Mr. Pridgin Teale, in a letter on the suction of cataract, refers to my opinion and experience as published in 1875. Will you allow me to say that since the date in question I have continued to employ suction, both in hospital and in private practice, and that I am entirely satisfied with the results obtained? The statistics of the Ophthalmic department of St. George's Hospital were not accurately kept prior to the appointment of an ophthalmic registrar in Dec., 1877; so that I can only appeal to records for two years. In these years, 1878-79, twenty-seven opaque lenses were removed, either entirely or partially, by suction. Four patients with laminar cataract had both eyes treated at once. Three cases were complicated with advanced interstitial keratitis. In one case opacity of the lens followed acute cyclitis with dropsy of the vitreous. Five were traumatic cases, and in three instances suction was applied for the removal of cortical *débris* left behind in extraction.

With one exception, the whole of these cases did perfectly well; and the exception had nothing to do with the use of suction. The patient was a man fifty-five years old, who had been wounded near the ciliary region by a pointed stick. His traumatic cataract was removed, a week after the injury, mainly by a downward iridectomy and a scoop, but some small portions were taken out by suction. The eye remained irritable, and the appearance of threatening symptoms in the other led me to perform enucleation.

Permit me to add that, in the main, I cordially agree with what Mr. Teale has written; but that I employ, and in my own hands do not share his objection to, a curette with notches at the margin of its opening.—*Lancet, Jan.* 10, 1880, *p.* 61.

75.—SCLEROTOMY IN DIFFERENT FORMS OF GLAUCOMA.

By Dr. L. De Wecker, Paris.

In the latest publications on the cure of glaucoma by sclerotomy (Mauthner), I have been reproached for not having upheld with more vigour an operation of which I am the originator. Great surprise has been created, especially by a passage in my Thérapeutique Oculaire, in which, addressing young surgeons who had not yet entered upon practice, I advised them to confine themselves to iridectomy, until some daring operator should succeed in demonstrating that we ought to give the preference to sclerotomy, and above all until it had been established upon practical grounds in what forms of glaucoma sclerotomy should be preferred.

Mauthner has already in part commenced to supply this demonstration. But while he proposes the almost absolute substitution of sclerotomy for iridectomy in the treatment of glaucoma, I have thought it right to advance towards this change of practice by selecting cases in which sclerotomy could be performed with facility, and then those in which it presented evidently fewer dangers than iridectomy, and finally cases in which the new operation offered serious advantages as regards the optical result, whilst at the same time it displayed not only an efficacy analagous to that of iridectomy, but even an incontestable superiority under certain circumstances.

The great object was to allow the new operation to become ripe, in order that it might be able to take the place of the old one from which it originated, and which has been a true blessing to humanity, although its results have not always been satisfactory, and in some exceptional cases have even been most unfortunate.

I commenced accordingly by substituting sclerotomy for iridectomy in cases of absolute glaucoma with atrophy of the iris, where the latter operation would have been illusory. In the second place, I thought sclerotomy might be preferred in the hemorrhagic forms of glaucoma, precisely where the discoverer of the operative treatment of glaucoma himself had feared to recommend his operation, so disastrous did the results show themselves.

Since the recommendation above referred to, I have experimented with sclerotomy upon a large scale, and I have gradually extended its indications, so as sensibly to have approached the

wise opinions of Mauthner. But still I have always selected cases in which the substitution of sclerotomy was justified by the obstacles to iridectomy, which obstacles were obviated by the new operation.

In my Chirurgie Oculaire I recommend sclerotomy in the following conditions. 1. In the incipient stage of glaucoma when, between the attacks, vision is normal. There is no doubt but that the formation of an artificial pupil in such eyes, even when completely successful, gives rise to an imperfect optical apparatus, causes dimness, and frequently astigmatism; and the patient, comparing the vision left him after the operation and that which he previously enjoyed, is always more or less discontented, although he may possess a central acuity of vision which is perfect. 2. In cases of simple glaucoma, in which both central and peripheral vision are nearly normal. The same considerations as to the deteriorating action of iridectomy are equally applicable here. 3. I recommend recourse to sclerotomy when, in simple glaucoma, the margin of the visual field, even at one side only, touches slightly or comes very near to the point of fixation. Every clinicist is aware that in such cases the iridectomy is very frequently followed by a further diminution in the field of vision, which may deprive the patient of central vision, a danger not to be feared with sclerotomy.

Having up to the present time treated a very large number of cases by sclerotomy, I think it is possible to give to the indications for the operation all the desired precision. I am of opinion, in short, that in every form of chronic simple glaucoma, in which the results of iridectomy have been much less brilliant than in the irritative forms, iridectomy should be definitely given up and sclerotomy exclusively employed. This important substitution appears to me justifiable, not only because sclerotomy possesses, according to my experience, a greater efficacy in such cases, but also because the proceeding is free from some inconveniences which, in a certain number of cases, have been observed to attend iridectomy.

By degrees I have succeeded in extending also the use of sclerotomy to cases of irritative glaucoma, in which the use of myotics transforms the disease into the simple form, and where well-marked myosis is produced by eserine. According to this statement, iridectomy would for the present be reserved solely for attacks of acute glaucoma, and for irritative forms in which the tension is such that the myotics do not produce any effect, or but an imperfect one. It will be noticed that these are precisely the cases in which an exact performance of sclerotomy would present great difficulties, and where, on the other hand, as in attacks of acute glaucoma, the most imperfectly performed iridectomy may prove efficacious.

As Schweigger has observed, we have in the use of eserin—several instillations of which should precede each glaucoma operation—the means of deciding upon the method which is to be preferred in each case. On any glaucomatous eye in which eserin acts well, sclerotomy should be performed; but on any eye in which the effect is not complete, and on which even the prolonged use of the myotic does not modify the form of the glaucoma, it is desirable to have recourse to iridectomy.

In order to form an opinion as to the curative effect of sclerotomy, it is necessary that it be most accurately performed. With regard to this point, I shall not deny that if the anterior chamber be shallow (which need not prevent the perfect action of the eserin), this operation might present some difficulty, whilst iridectomy would be more readily performed. But I am so convinced that it is sclerotomy which in such cases will in future be demanded, that I do not fear but that routine and a greater operative facility will here also give sclerotomy the position it deserves.

In all the sclerotomies which I have lately performed, I have used a narrow Graefe's knife, which I found more suitable in cases of shallow anterior chamber than the sclerotome which I formerly recommended. Moreover, as also Mauthner has recommended, I do not confine myself strictly to the formation of a flap two *millimètres* high, of which the middle third is left undivided. But, especially if the depth of the anterior chamber permit it, I form a flap of from three to four *millimètres* high. The knife should penetrate the sclerotic very precisely at a distance of one *millimètre* from the clear cornea, and then, the blade being held perfectly parallel to the plane of the iris, it should be passed very slowly in front of the latter, so that the counterpuncture may fall also exactly at a distance of one *millimètre* from the internal border of the cornea. If the blade be not held quite parallel with the plane of the iris, there is danger that it may pass through the cornea, and that the sclerotomy may be but half performed; or, if it be directed too deeply, it may come out through the sclerotic at an exaggerated distance from the cornea, may wound the ciliary body, and may provoke troublesome hemorrhage and irritative symptoms of an alarming kind. A complete curative effect should not be expected from sclerotomy unless it is performed strictly *lege artis;* and, in order that this may be, it is necessary that the operator should have the proceeding at his fingers' ends. I would like every practitioner to be convinced that it is only after a certain apprenticeship that good and precise sclerotomies can be performed. The section should be made by slow sawing movements, leaving exactly one-third of the flap undivided.

Before giving the results obtained by this operation, I wish to draw attention to the difference which exists between sclerotomy and simple paracentesis or puncture of the sclerotic, as Mackenzie performed it or recommended it. In an eye upon which sclerotomy has been precisely performed, the anterior chamber is not reformed until the fifth or sixth day, and often still later, although a bandage be kept applied to the eye; while, as we know, the aqueous humour is collected very rapidly after simple paracentesis.

Without any cystoid cicatrix having been formed, the subconjunctival tissue remains often œdematous for a long time, and the neighbourhood of the wounds especially remain swollen. Moreover, I have observed that in some eyes on which very large sclerotomies had been performed, with flaps of from three to four *millimètres* in height, and also in cases where the uncut bridge had been very narrow, the cornea presents depressions close to its margin and in front of the incisions. This sinking in of the corneal tissue gives very much the appearance of ulcerative facets, but differs from the latter in the dryness which the epithelium presents. The nature of these appearances is evidently not such as to make me abandon my opinion as to the curative action of sclerotomy; on the contrary, they confirm me more and more in the idea that it is the increased facility which is given to the process of filtration that is to be regarded as the chief factor.

In describing the results which I have obtained from sclerotomy, I am especially anxious to point out in what way they differ from those of iridectomy. In the first place the new operation admits of much greater freedom in the after treatment. The patients being provided with a bandage, may return home, and can, for the most part, resume their occupations upon the second or third day afterwards.

Having examined the acuteness of vision of the greater number of my patients upon the day following the operation, and the field of vision two or three days afterwards, I am in a position to state that an improvement was evident in most cases, and that in no instance was there a deterioration of vision. In one case, where I had to deal with a chronic irritative glaucoma, which, when the other eye was found affected with retinal apoplexies, was recognised as of a hemorrhagic nature, the vision after the sclerotomy was found considerably diminished in consequence of a hemorrhage situated at the macula lutea; but yet this disturbance of vision was of short duration only.

When, after sclerotomy, the anterior chamber is only slowly re-established, an alteration in the refraction of the eye may be observed, owing to displacement of the crystalline lens; but

we have not found astigmatism to become developed, as often happens after iridectomy, putting the patient in a very uncomfortable position. In short, sclerotomy, except for a slight transitory change in the refraction, leaves the operated eye absolutely intact, with but a short period of irritation.

I mention, in my Thèrapeutique Oculaire, the case of a medical man upon whom I performed sclerotomy for simple chronic glaucoma, which had damaged his vision so much that he was unable to count fingers at more than a *mètre* and a half (nearly five feet). The defect of the field of vision at the inner side approached close to the point of fixation. This operation was performed in November 1877, and I had an opportunity of observing its effect in May 1879, when the patient came to request that I would perform the same operation upon his other eye, although it possessed perfect acuteness of central vision and an unblemished field of vision. Believing that he had felt some premonitory signs of glaucoma in the second eye, he wished to have the sclerotomy performed in order that he might leave Paris without fear. I could not refuse him this operation, for, in the eye which had already been operated on, there was an enlargement of the field of vision and an improvement in the central vision, fingers being now counted at two *mètres* and a half (eight feet and a quarter). This patient's case allowed me to satisfy myself again how very little visual trouble is caused by sclerotomy, for the sight was absolutely good from the second day after the operation.

Since the commencement of the year 1879, I have performed forty-eight sclerotomies, most of which have been noted with great care by Dr. Manvarsen of Bucharest for an essay on the subject. These numerous operations confirm me in the opinion already expressed—namely, that in the various forms of chronic simple glaucoma, sclerotomy is not only as efficacious as iridectomy, but that it is far superior to the latter, being free from the disagreeable effects of the excision of a portion of iris.—*British Medical Journal, Nov.* 22, 1879, *p.* 809.

76.—ON SCLEROTOMY IN GLAUCOMA.

By H. R. SWANZY, Esq., M.B., F.R.C.S.I., Professor of Ophthalmic and Aural Surgery, Royal College of Surgeons in Ireland.

You are aware that twenty-two years ago the cure for the disease called Glaucoma was discovered by my lamented master, Albrecht von Graefe. Until then this disease was absolutely incurable, and if Von Graefe had not contributed in any other way to the progress of ophthalmology, this discovery alone would have immortalized his name. By means of it hundreds,

nay thousands, are annually relieved of a disease which otherwise leads with certainty to complete blindness, attended in many cases with violent and protracted pain. The cure which Von Graefe discovered for glaucoma is iridectomy. An incision is made in the corneo-sclerotic margin, opening the anterior chamber; through this incision a small portion of the iris is drawn out and abscised. The prominent symptom in glaucoma is increased tension or hardness of the eyeball, due to its being over-full; and this it is which causes the blindness and pain by pressure upon the optic nerve and ciliary nerves respectively. After a properly-performed iridectomy the tension of the globe is permanently reduced to the normal standard in a large number of cases, the pain relieved, and the sight preserved. What causes glaucoma? How does increased tension of the eyeball arise? How does iridectomy cure this disease? To these questions the great author of the cure himself failed to find satisfactory answers. Besides him many physiologists and ophthalmologists have endeavoured to solve these problems, but it is only within the last two or three years that the true light seems to be breaking through the cloud. It does not come within the scope of this paper to give even a cursory view of the various doctrines of glaucoma which have been held from time to time. I shall merely mention that these theories followed in one or other of two chief lines, one which regarded glaucoma as a disease caused by hypersecretion of the intra-ocular fluids, the other which regarded it as caused by retention of those fluids, or obstruction to their passage out of the eyeball. Evidence has of late been running very strongly in favour of the latter view. Now, amongst those who long ago declared themselves as supporters of the theory of retention was De Wecker, of Paris, and not only did he support this view, but he also declared that in his opinion the iridectomy acted in the cure of glaucoma, not by virtue of the bit of iris excised, but by virtue of the cicatrix in the corneo-sclerotic margin. The microscope shows that this wound does not heal by the immediate approximation of its lips, but that a semi-transparent substance is interposed. De Wecker believed that this substance admitted of a freer transudation of the intra-ocular fluids, and thus the cure. Since De Wecker put forward this view, which for many years he defended single-handed, my friend, Prof. Theodore Leber, of Göttingen, has made his famous investigations with regard to the "Interchange of Fluids in the Eyeball." By these he has shown that the point at which the effete intra-ocular fluids chiefly escape is at the angle of the anterior chamber, passing outwards through the ligamentum pectinatum, or space of Fontana into the canal of Schlemm. Pathological researches (Adolf Weber, Knies,

&c.) of a still later date have shown that in glaucoma this way of exit becomes obstructed by the adhesion of the periphery of the iris to the periphery of the cornea. These investigations combined now to make De Wecker's theory of the action of the corneo-sclerotic cicatrix seem very plausible, and it remained only for him to establish his position by an operation which would enable him to produce this cicatrix without an iridectomy. The great difficulty to be contended with was the tendency of the iris to prolapse and become incarcerated in the incision, a circumstance which would of course completely nullify the desired effect. Fortunately eserine, the active principle of the Calabar bean, was isolated just at the time, and is now an article of commerce. A drop or two of a one per cent. solution of eserine produces an active contraction of the pupil, so that when the incision in the corneo-sclerotic margin is completed no prolapse of the iris takes place. In order the more completely to guard against this occurrence, De Wecker performs the operation which he calls sclerotomy in the following way: With Von Graefe's cataract knife a flap of from three to four millimetres high is formed in the upper portion of the corneo-sclerotic margin, the line of incision lying one millimetre distant from the clear cornea. The flap, however, is not completed, but about one-third of it is allowed to remain undivided, and the knife is then slowly and carefully removed from the eye. During the performance of the operation the aqueous humour escapes, but, thanks to the violent contraction of the pupil from the eserine previously instilled, and to the bridge left standing, prolapse of the iris does not take place. In a paper by Dr. de Wecker, at the late meeting of the British Medical Association at Cork, he reports that he had performed forty-eight sclerotomies, all with good result, since the commencement of the year. The cases upon which he had operated were all cases of simple chronic glaucoma. This is the most insidious form of the disease. Exteriorly the eyes seem, for the most part, quite healthy, there are no so-called inflammatory symptoms, no pain or injection of the eyeball. The chief—indeed, I may say the only—objective symptom is gradually progressive blindness. Upon examination, the surgeon finds increased intraocular tension, a deeply excavated optic disc, diminished central vision, and contraction of the field of vision. Now in this form of the disease iridectomy has not proved so successful as in the more acute or inflammatory forms, and of this Von Graefe himself was well aware. While many cases of simple chronic glaucoma are arrested in their progress by iridectomy, still there is a considerable percentage of cases in which beyond

doubt the downward progress is even more rapid after the operation than before it.

These are some of the most anxious cases with which the ophthalmic surgeon has to deal. The very worst cases of simple chronic glaucoma are those in which the contraction of the field of vision approaches very close to the fixation point, while perhaps the power of central vision may be fairly normal. Let such a case alone, and the central vision will ere long become involved, and absolute blindness rapidly ensue, whilst if an iridectomy be performed the prognosis still is doubtful, and great is the disappointment if soon after the operation the vision deteriorates at a more rapid rate than before. My experience with iridectomy in such cases would make me very satisfied if I succeeded in securing for the patients the poor bit of sight they may have prior to the iridectomy—for an improvement in it I have never looked.

The whole number of the sclerotomies which I have as yet performed has been five. This may seem to be a very small number, but chronic simple glaucoma is not a very common disease. So far as it is possible to judge, all these operations have been successful. Of course the great question is, will the effect which has lasted some months or weeks continue for years? and the answer can only be given after years. As yet I have not employed sclerotomy for any more acute forms of glaucoma, not but that it would in all likelihood be as useful there as in the chronic simple form, but because iridectomy is a most admirable remedy for the acute form, and it is not good to forsake an old ally who has always been faithful, and also because the anterior chamber in cases of acute glaucoma is often very shallow, and would make sclerotomy impossible. I do not think sclerotomy is an easy operation to perform accurately; it is without doubt a much more difficult one than iridectomy. It is well, therefore, that each one who ventures an opinion upon it should make sure that he performs it rightly. From the descriptions I have read I have no doubt but that some operators have done the proceeding scant justice in this particular. One surgeon habitually passes his knife through the iris and ciliary body; others find it almost impossible to avoid extensive prolapse of the iris. Every case of chronic glaucoma is not suited to the operation. I would not, so far as I can see, perform it in any case in which eserine did not act well upon the pupil. In none of my cases was there any inflammatory reaction after the operation, nor any irritation of the eyeball. A slight vascular injection in the neighbourhood of the wound was the most that was ever present.

The chief point which I wish to bring before the Society is the fact that in sclerotomy we have, as it seems to me, a much

better remedy for chronic simple glaucoma than in iridectomy; and I think it is very probable we might find it useful in acute glaucoma; but inasmuch as iridectomy is such a capital remedy for it, and as it seldom forsakes us, it would be wrong to apply any other. However, for chronic simple glaucoma it certainly seems to me another remedy was very much wanted. We are supplied with it now.

Dr. Jacob thinks it is an operation that might be called a paracentesis. After all I think there is no great objection to call it paracentesis; but by that word we mean something with respect to the eye which has only a temporary effect. We use it very frequently, but it is employed through the cornea, and in this operation the great point is that the incision should lie in the corneo-sclerotic margin, *i.e.*, where the fluids escape out from the eyeball, and where we want to make a way for them artificially to escape whence they are pent up.—*Medical Press and Circular, Dec.* 10, 1879, *p.* 499.

77.—ON THE DIAGNOSIS OF TOBACCO AMBLYOPIA.

By Edward Nettleship, Esq., F.R.C.S., Ophthalmic Surgeon to St. Thomas's Hospital.

It seems desirable to call attention to certain symptoms which occur with remarkable constancy in cases of tobacco amblyopia, and which, though well known to many Continental observers, have not received the attention in this country that their diagnostic value warrants. The symptoms alluded to all depend on the fact that in this amblyopia the central region of the visual field is the part first affected, and remains throughout, the seat of greatest relative defect, and, further, that the periphery of the visual field remains of full size.

1. However much acuteness of central vision may be lowered, the patient shows no difficulty or awkwardness in walking about, and has not the "amaurotic aspect" so common in the subjects of progressive atrophy of the nerves, in whom peripheral contraction of the field is an early event, and may lead to difficulty in avoiding obstacles and to the other inconveniences which arise from loss of indirect sight, before central vision is much affected.

2. The sight in tobacco amblyopia is nearly always best in a rather dull light, the complaint being very much the same as in early uncomplicated nuclear cataract. The patients, when asked whether the sight is always bad alike, generally say that it is better the first thing in the morning and again towards evening, that it is better on dull days, and that in direct sunlight it is very bad. This symptom is noted as present in nearly all the cases related in this paper. It is no doubt, as in nuclear

cataract, related to the size of the pupils, sight being rather better when they are slightly dilated. It does not depend upon any periodical changes in the state of the nerve or retina, and that it is really connected with the pupil is shown by one case, in which, with pupils which were motionless to light, this symptom was entirely wanting. We have usually to be content with patients' statements on the subject, but it is sometimes possible to prove the fact with accuracy by testing the sight with types in full daylight, and again with the blinds down, or in a dark room by gaslight. The symptom may probably, so far as acuteness of central vision is concerned, be explained by the disproportionate contraction of the pupil which occurs when the whole eye is exposed to bright light, as in full daylight or when looking at a bright lamp flame. The contraction of the pupil which then occurs is the result, first, of the light which enters the pupil from the object looked at, and, secondly, though to a less extent, of light which reaches the retina obliquely through the pupil, and indirectly through the so-called opaque structures, iris and sclerotic. The dilatation which always ensues when the back is turned to the window, or when the eyes are shaded by the hand, although the illumination of the object looked at undergoes no change, or is even increased, is a familiar illustration. The exclusion of indirect light by causing dilatation of the pupil allows more light to enter from the object looked at, and thus adds to the brightness of the retinal image formed on the yellow spot.

The condition under which this advantage would be most appreciated would evidently be one in which the function of the central part of the retina was defective, and this is precisely the condition in tobacco amblyopia; for in this malady a dulness of vision, usually well circumscribed, can be shown by means of the perimeter to exist at the central part of the field, and hence the corresponding part of the retina, the yellow-spot region, needs every advantage it can derive from brighter illumination. The case is strictly analogous to that of nuclear cataract, for in both the improvement which follows partial dilatation of the pupil depends on the brighter retinal images thus secured at the central part of the retina. In very advanced cases of tobacco amblyopia it is possible that incomplete atropine mydriasis might give some slight improvement, just as in incomplete, but well-advanced, nuclear cataract; in the less severe cases its effect on accommodation would more than counterbalance any such advantage.

The symptom now considered is not explained by any widening of the field of vision which might be supposed to occur when the pupil dilates. In the first place, were any notable widening of the field demonstrated as the result of slight dilatation of

the pupil, this would in no way affect the acuteness of *central* vision, but would simply, in proportion to its extent, improve *indirect* sight. It appears, however, that in fact the effect of any considerable enlargement of the pupil on the size of the visual field is, under ordinary circumstances, counteracted by the relaxation of accommodation that occurs with it; so that practically neither wide dilatation by atropine, nor extreme contraction by eserine, alters the extent of the field; the advancement of the anterior surface of the lens in accommodation causes a widening of the field, which counterbalances the contraction of the field caused by the associated myosis. It is, perhaps, possible that the enlargement of the field for separate colours which has been shown to occur with wide dilatation of the pupil may affect the result in some of the cases under consideration.

The symptom here described has been said to be typical of alcoholic as distinguished from nicotic amblyopia; the latter affection being, according to some observers, marked by great contraction of pupils and improvement of vision in bright light. The question, so far as we are now concerned, is entirely one of names; whether alcohol or tobacco be accepted as the essential cause, all are agreed that cases of symmetrical progressive amblyopia, with marked myosis and vision better in *bright* light, are far less common than the cases in which the pupils are normal (or occasionally sluggish or a little dilated), and sight is better by *dull* light.

3. The state of the visual field. In tobacco amblyopia the greatest functional defect is always found to occupy an oblong or oval patch, which extends from the fixing point (corresponding to the yellow spot) out towards, and often beyond, the blind spot (corresponding to the disc). This patch ("central scotoma" of authors) is only defective (amblyopic), never blind (amaurotic); it is a "relative" not an "absolute" scotoma. Indeed, it is so relative that the patients do not often make complaints that would lead us to suspect its existence, whereas, in the absolute scotoma caused, *e.g.* by a large hemorrhage at the yellow spot, the symptoms are usually very suggestive of the state. Still, in many tobacco cases we notice that the patient in attempting to read, moves the print about as if trying to find some position in which it is better seen; this symptom is best seen in the early and slight cases, when print of moderate size (say 10 J) can still be read, and the patient, not having got accustomed to the defect, is constantly trying to see smaller print.

Cases of amblyopia with central scotoma have been well known since Graefe's time, and their natural history in great part written. Their close association with tobacco has, how-

ever, only been clearly insisted on more recently, especially by Förster and Hirschberg. There has been too great a tendency on the part of observers in this country to speak of the eye symptoms caused by tobacco as "amaurosis" and "atrophy." Such terms have proved great stumbling-blocks to the general acceptance of tobacco as a common cause of failure of sight, and to the association of the well-known cases of amblyopia from central scotoma, with their true cause.

This central scotoma is, as Förster and Hirschberg have shown, although evident enough for white, especially apparent for those colours which are perceived only in the central part of the field, and especially for green and red, the former appearing white or grey, and the latter brown or "no colour," or "dull," according to the exact shade and tint employed, the intensity of the defect, and the descriptive power of the patient. In most of the cases the area of the scotoma is smaller than the visual field for the central colours, green and red, and hence a zone is present at some little distance from the centre on which these colours are well seen. The size, and it may be added the *intensity*, of the scotoma differs considerably, being in general terms proportionate to the state of central vision; the larger the scotoma the worse the sight. Size and intensity of the scotoma do not, however, always go hand in hand; scotomata of different intensities may be of equal size. These variations in the extent of the defective area, account to a great extent, if not entirely, for the differences observed in respect to colour blindness in different patients, and for the discrepancies amongst observers on this point. By many, probably most, of the patients, colours are well perceived if seen in large patches, whose images occupy a large space in the visual field, and pass beyond the limits of the colour-blind area; but the colour of small coloured objects (*e.g.* gold and silver coins) is often mistaken. In regard to this symptom, apparent defects are sometimes dependent on the want of observation and intelligence in the patient, and it must be remembered that many men are naturally somewhat defective in colour perception without being technically "colour blind."

The scotoma of tobacco amblyopia is invariably symmetrical in the two eyes, indicating affection of parts of the retinæ or nerves which correspond anatomically, but are not physiologically identical; a fact which is strongly in favour of the symptoms being due to a change in the orbital part of the optic nerves, and not to any cerebral or central lesions; and it is conjectured that the change may chiefly occur in the fibres at the periphery of the nerve, which are believed to supply the central part of the retina.

In the great majority the scotoma extends outwards, almost horizontally, but with a slight downward tendency; in a few it seems to have a much more oblique downward direction. In many of the early cases, however, where the scotoma is very small and only faint, it is most marked just below and to the outer side of the fixing spot, and quite absent just above this spot; it would appear, therefore, that it does not begin *precisely* at the fixing point.

In a few cases the scotoma is quite central, surrounding the fixation spot about equally on all sides, and not extending specially towards the blind spot. This form of the scotoma has been thought to occur in cases of amblyopia caused solely by alcohol. I have only identified such a scotoma in one or two cases as yet, and in these the patients were smokers.

The periphery of the visual field has, whenever I have taken it, been normal, showing no contraction, a fact which agrees with the observations of others. In respect to the fields for green and red this point is much more difficult to determine owing to a variety of circumstances, and I place but little reliance on such of my own observations as show a slight apparent peripheral contraction or irregularity.

It is not of course pretended that accurate measurement of the field of vision is commonly necessary for the correct diagnosis of tobacco amblyopia. The value of such troublesome examinations, apart from the scientific interest they possess, lies for practical purposes in the fact that they emphasize the value of some of the more easily appreciable symptoms which have been mentioned, and which, though characteristic enough, may be overlooked unless sought for. These are, besides the improvement of vision in dull light, the history of equally symmetrical failure of sight without other ocular symptoms, and without cerebral or spinal symptoms, excepting often nervousness and tremulousness; the presence in many cases of some degree of colour blindness for green and red, and sometimes for yellow, much more noticeable if the coloured objects be small spots, and frequently passing undetected if large masses of colour be used for testing; the failure of vision, generally gradual, but occasionally rapid, or at any rate noticed suddenly; the pupils natural in size and activity, or at most rather sluggish and somewhat too large. If it be added that there are no signs of chronic glaucoma, nor of incipient cataract, that the ophthalmoscopic changes are very slight in comparison with the defect of sight, being limited in most cases to (1) congestion and (2) more or less pallor of the optic disc alone, and that spectacles suitable for the patient's age and refraction do not bring sight up to the normal, we shall see that the diagnosis of this disease usually presents no difficul-

ties, and that it might often indeed be made even without ophthalmoscopic examination. It need hardly be added that though in nine cases out of ten a diagnosis arrived at without the ophthalmoscope would be correct, this examination should invariably be made in order to exclude the other causes of similar symptoms.

Into the vexed and difficult question of how, and how much, the sight is affected by alcohol I do not propose to enter in the present paper. It may be noted in passing, however, that the more carefully the subject is examined the less reason do we find for holding that alcohol can of itself directly produce ocular symptoms like those caused by chronic tobacco poisoning. Indeed, affections of sight capable of being easily confused with those which form the subject of this paper may, I believe, be sought for almost in vain among non-smokers. It is no doubt rare to see the disease in smokers who are water-drinkers, and very common to see it in those who have damaged their stomach and nervous system by drink. But the more we study facts like these in cases, the stronger does the evidence seem that without tobacco this common and characteristic form of amblyopia would be almost unknown. In respect to treatment, it is before all things essential to believe in the advice we give; general directions to "smoke less and drink less" are, as a rule, useless. Complete permanent abstinence from tobacco must be insisted on, for though something short of this would, as many cases show, often be enough, it is quite unsafe to give any discretion to the patient in respect to so insidious a habit.—*St. Thomas's Hospital Reports*, vol. ix., *p.* 51.

DISEASES OF THE SKIN, ETC.

78.—ON THE HEALING OF BURNS AND ULCERS BY THE UNION OF GRANULATIONS.

By JOHN DUNCAN, Esq., M.A., F.R.C.S.E., Surgeon to the Royal Infirmary, Edinburgh.

[The patient was a healthy young man who had been scalded by the bursting of a boiler. Several extensive burns existed.]

You have doubtless heard of the process of healing by the third intention, by the union of granulations; but practically it is not often brought under your notice. Nevertheless, in the healing of large wounds, as after amputation and excision, it undoubtedly plays a large part; and it is sometimes, though rarely, utilised when plastic operations fail to unite by the first intention. It was this process that I called to my aid. It was possible, the skin of the trunk being loose, to bring the margins of the sore

into contact by forcible pressure. The point that remained doubtful was our ability to maintain them in this position sufficiently long to allow the union to be consolidated. Ordinary sutures would undoubtedly cut their way out in a day or two. Many years ago, I made an unsuccessful attempt with hare-lip pins. I determined now to employ the button-suture.

Having washed the sore carefully with boracic lotion, I operated under the spray, because, after the parts are drawn together, the wound ceases to be a superficial ulcer. I then entered the needle two inches from the sore, and brought it out at the margin, entered it again at the opposite margin, and brought it out two inches off. When the sutures had been drawn tight upon the buttons (of the largest size), the sore, originally three inches in diameter, was reduced to a line three inches long. It was then dressed with carbolised gauze. The dressings were changed on the fourth day, and, on the ninth, I removed the buttons, the sore being healed with the exception of little marks where the silver wire passing from the buttons had slightly cut the skin.

Encouraged by this success, I now attacked the second sore, which is situated on the shoulder. It was two inches and a half in diameter, besides new cicatrix about half an inch wide. One or two lessons were taught us by this operation. I folded up the wound with my finger and thumb, and then passed a suture deeply under it, and drew it tightly over two large buttons. On removing the manual pressure, it was at once evident to you that this method was not nearly so effectual as the one previously described, in which the skin only on each side was involved in the suture. The folded-up sides of the sore, instead of lying in contact, formed a deep hollow—assumed a V-shape. To remedy this, I passed sutures above and below by the former method, and so succeeded in bringing about exact apposition. Thinking that by the first suture the tension must have been greatly relaxed, I used very small buttons for the upper one.

It has not been found necessary to change the dressing till now, the seventh day after the operation, and you see the result. The upper suture has cut itself out, and is lying quite loose. The part of the wound where it was inserted has diminished in breadth by half an inch, but the line which the wire has cut has been added to it. Where the lowest large buttons were applied, the burn is healed, and opposite the central deep suture it is reduced to half an inch in breadth. We have therefore gained much ground, but not nearly so much as in the former remarkably successful operation, and, I think, entirely because the manner of performance was less efficient. I have treated in the same way, and with perfect success, two other ulcers of

smaller size than these, one of a syphilitic character on the forearm, the other a strumous sore on the neck.

It is evident that a method which shortens the healing of a wound by weeks, as in the case you have before you, is of the greatest possible value. And I believe that it may also present other advantages. In burns, for example, it may sometimes be used to prevent contraction by approximating the margins in a direction which will not be injurious. The mere diminution in the amount of cicatrix is in itself a benefit.

It is not difficult to appreciate the circumstances under which this proceeding may be practised. It is essential, in the first place, that the granulations be brought into a thoroughly healthy state by medication suitable to the particular case. It seems to me that the somewhat exuberant hyperæmia which results from the use of antiseptics is very favourable to a successful issue, if it be not permitted to go too far. Constitutional treatment is, of course, necessary as a preliminary to any interference with constitutional sores.

In the next place, the sores must be so situated that the margins can be brought into apposition. Plainly the trunk of the body is in this respect most favourably circumstanced. But in any part, if this condition be fulfilled, whatever the pressure employed to do so, I believe the result will be good. You saw that I had here to use decided manual force.

Some surgeons hasten the healing of obstinate ulcers of the leg by making incisions around them. This appears to do good, partly because it increases the area and source of epidermic growth, partly because it brings within the range of contraction a larger quantity of movable skin. I think it possible that, in some cases, a combination of these operations might be made, the possibility of perfect adjustment being always presupposed.

Lastly, it is plainly necessary that the apparatus used be capable of sustaining the tension without producing ulceration. The buttons should be of the largest size, at least an inch long; and I have found the greatest benefit from the little projections on the side which Mr. Lister has recently added.—*British Medical Journal, Oct.* 18, 1879, *p.* 603.

79.—ULCERS.—MARTIN'S RUBBER BANDAGES.

By H. H. CLUTTON, Esq., M.B., F.R.C.S., Assistant-Surgeon to St. Thomas's Hospital.

Martin's rubber bandages for the treatment of ulcers have now been used in England for more than a year, and yet very little of the experience obtained by English surgeons has been reported. They were first brought publicly before the notice of our profession by Mr. Hutchinson at the meeting of the British

Medical Association in August 1878; and in October of the same year Mr. Callender exhibited at the Clinical Society two cases which had been treated in this way.

Dr. Martin, in the British Medical Journal of the same month, gave a detailed description of their mode of application and the class of cases in which a good result might be expected. And on Dec. 14, 1878, another letter appeared from the inventor describing the proper quality of the material, the different sizes and degrees of thickness which should be used in different cases. Mr. Horne in the same journal for April 5, 1879, gives us his experience in the treatment of twelve cases. It is noticeable that although he treated them all in bed they all of them made slow progress; they advanced most rapidly up to a certain point and then seemed to stop short of healing. Perhaps experience will teach that with the rubber bandage rest in bed is not "the best position for improvement." However that may be, rest in bed is certainly not necessary, as was strongly insisted upon by Dr. Martin.

Mr. Horne's failure would rather suggest that either the ulcers were peculiarly unfavourable, *e.g.*, completely encircling the leg, or that the complete rest in bed was not an advantage but the reverse. Dr. Martin attributes Mr. Horne's failure, and others of which he says he has heard, entirely to the bandages employed, which were English made; but other surgeons who have tried the English bandages have not met with these failures. He states that he has "no hesitation in asserting that the disappointment experienced by Mr. Horne was due to the presence of carbolic acid in the bandages he employed." He is also "informed that carbolic acid is used largely in the English manufacture of rubber." On the other hand the manufacturers themselves in London state that such an adulteration is impossible, and that nothing but pure Para rubber, with the addition of sulphur in varying proportions by different manufacturers is ever used. The English bandages have also been tested for such an ingredient in their composition, but the carbolic acid has not been found. Dr. Martin also enters into a long discussion arguing against the quality of these bandages, and asserting that in no respect are they equal to the American. All this may be perfectly true with respect to some of the English bandages, and even if it were true with regard to all, yet, if success is obtained with these bandages, Dr. Martin will, I am sure be more than satisfied.

In the out-patient rooms of our London hospitals there have been ample opportunities for testing the efficacy of these bandages, and I am sure sufficient experience has been obtained to warrant us in giving an opinion as to their utility. I have been surprised at the result. I must confess that there is some

difficulty in keeping these patients under observation long enough to witness the ultimate success of the treatment—the perfect cicatrisation of the ulcer. They see no object in losing time by waiting to see the surgeon in the out-patient room after they have lost all pain, can stand all day, and their ulcers are diminishing in size. Consequently as they learn to treat themselves they discontinue coming, and we are unable to say how soon the ulcer heals, and to what extent the patient has been benefited should it not have healed. Now and then they return to show themselves, expressing the greatest gratitude for the relief they have obtained, and these are generally the worst and most chronic cases that we see in the out-patient room. They are simply astonished at the effect produced by such simple means after years of suffering and incapacity for work. Patients with ulcers of only short duration do not trouble themselves to return. To be cured at once of their troubles is nothing more than what they expect, at least this seems the most reasonable explanation of the fact that only the worst cases return to report progress.

For a full description of the method of applying the bandage, and the different classes of cases suitable for this treatment, the reader is referred to the article alluded to above, written by Dr. Martin (*Retrospect*, vol. lxxix., p. 233).

To describe it shortly, the treatment consists in applying a solid rubber bandage to the limb affected, before getting out of bed in the morning, without any application whatever intervening between the limb and the bandage. It is put on with only just sufficient degree of tightness to prevent its slipping down. It is kept on all day and only taken off at night on going to bed, when it is then washed in warm water, and hung up to dry. The ulcer is then covered with some simple dressing, which is kept in contact by a few turns of an ordinary roller. All greasy applications are best avoided, as they soon have a deleterious effect upon the rubber, unless very carefully removed in the morning before applying the bandage.

All ulcers can be treated on this principle, but the varicose ulcer is the one more especially benefited by this method. The syphilitic ulcer, of course, requires the special treatment of the disease, but after a time the syphilitic ulcer often becomes as much a local disease as any other ulcer; the cure can then be completed by Martin's bandage. I will quote only one or two cases.

J. H., 39, carpenter, had suffered from ulcers of leg for twelve years; only once had they healed, and even then remained sound only for a short time. His left leg was covered with numerous circular superficial ulcers, and the cicatrices of former ulcers that had healed. On enquiry it proved that he had had syphilis

twenty years before, with well-marked secondary symptoms. He rapidly improved under iodide of potassium and careful strapping, but the ulcers then remained stationary, although the iodide was pushed to its limits. He continued much in the same condition for three months, doing no work at all, partly because it caused him so much pain and partly because he was strictly enjoined to remain in the horizontal position day and night. He was then ordered a Martin's bandage, and told to go to work. In a fortnight he returned in wonderful spirits. He said he would not be without the bandage for £5, that he had had no pain since its application, and that he could now stand all day and do his work. The ulcers had considerably diminished in size and were all healing. He did not again present himself at the out-patient room for many months, and he then only returned to show his gratitude; his ulcers had completely healed six weeks after the first application of the bandage.

C., æt 50, had for many years been the subject of varicose veins. An ulcer had formed three months before, which had completely prevented him from following his occupation. He could neither walk nor stand without great pain and swelling. A Martin's bandage was applied, and we did not again see our patient for many months. He then returned to say that he was quite well, that his ulcer had healed in a month, and, that so pleased was he with the result, that he had begun to treat his wife with the same bandage. She had had an ulcer for fourteen years, but he was now rapidly curing her in the same way.

J. B., æt. 63, timekeeper. His occupation obliged him to stand all day. He had had an ulcer for twelve months, which was gradually getting larger; he had also large varicose veins. A Martin's bandage was applied, and he was told to continue standing as before, and not to rest in any way. The ulcer was completely healed in five weeks.

E. W., æt. 50, for ten years had suffered from an ulcer of the leg, which presented extremely hard and callous edges. She returned a week after the application of the bandage and stated that all the pain had gone, and that she could now walk with comfort. She never visited the out-patient room again, but I presume that the result was in the end completely satisfactory, for, as a rule, the chronic ulcers which obtain little relief remain longest as out-patients, and are often a sore trial to the attending surgeon.

Now, as regards failures, many patients absolutely refuse to be treated in this way, and if you eventually succeed in inducing them to try it, they return, saying it is useless to persist as it causes them more pain than they had before. Others suffer

from an eruption similar to eczema and an itching on taking off the bandages such that they decline continuing its use. Those, however, who have been induced to try again have generally succeeded. Frequently the bandage is put on so badly by the patient himself that the pressure is anything but uniform, and the complaint is then often made that it always slips down. In such cases the obvious treatment is to see the patient put it on himself, and to criticise his skill in bandaging. But by far the greatest difficulty that one has to contend with in treating ulcers by this method is presented by the old lady with a chronic ulcer, who has been in all the out-patient rooms of the London hospitals, and has at last quietly settled down in one with the fixed determination of being treated with nothing but mist. alba and strapping. At the end of a long afternoon in the out-patient room one is hardly in a fit condition to argue the point with such a patient.

The patients that really seem to suffer pain worse than they had before the application of the bandage are those whose ulcers are rather inflamed and of recent origin. The old chronic ulcers are undoubtedly the most suitable cases for this treatment and derive the greatest benefit. However, the pain and eczema that sometimes follow have hitherto yielded to a continued use of the bandage. The English-made bandage, therefore, has not been so unsuccessful as Dr. Martin seems to fear. My own experience with them has been so good that I should hesitate before recommending hospital patients to incur the additional expense of buying the American bandages. In fact I have not used an American bandage at all, and have still to meet with an unsuccessful case. In the treatment of varicose veins, with or without ulcers, the advantages offered by the rubber bandage over the elastic stocking are so great that the latter will I think soon fall out of use. The elastic threads of the stockings in their application and removal are so soon broken that they exert a somewhat irregular pressure—they consequently cease to support the veins, and, in time, rather aggravate the disease, unless the patient can constantly afford to renew the same. The bandages, on the other hand, give a uniform pressure, and last, apparently, a long time; how long they absolutely will last is a matter of future experience. That they sometimes cause considerable irritation of the skin at first is well known, and Dr. Martin admits that this is the case with his own bandages. Whether this irritation is more frequently met with when employing English-made bandages than has been the case in America requires I think a longer experience. We certainly owe a great debt of gratitude to Dr. Martin for his valuable suggestion, for few improvements have made such a radical change in the out-patient room as this. The principle

is, of course, old, but the *best* application of that principle is quite new. For all this we are indebted to Dr. Martin. I cannot but hope, however, that this new fear as to the usefulness of anything but one particular variety of rubber, which costs more than any other, will not gain ground, for I am convinced that any such fears are groundless. The English-made bandages here alluded to have been obtained from Messrs. Walters, Palace Road.—*St. Thomas's Hospital Reports*, vol. ix, 1879, *p.* 197.

80.—NOTES ON THE TREATMENT OF ECZEMA.

By Dr. JAMES SAWYER, M.R.C.P., Physician to the Queen's Hospital, Birmingham.

In failure to recognise the general constitutional condition with which the affection of the skin is associated, or in failure to treat it successfully, usually lies the cause of failure to cure a case of eczema. Eczema is nearly always a local expression of one of several diatheses, with which it is linked in intimate causal relation. Some local irritation may determine or keep up the local outbreak, and be its proximate cause; but it is only a concurrent cause. The proneness to the local malady, its remote cause, the reason why the local irritation results in eczema is to be found in some general constitutional abnormality. In the treatment of eczema we should always search for, and, finding it, endeavour to remove, any sources of cutaneous irritation, such as scratching, the wearing of flannel next to the skin, uncleanliness, exposure of the affected part to the irritating action of heat, cold, water, urine, discharges, bad soap, or any mechanical or chemical irritant such as are met with in various occupations. But in the majority of cases no such external excitants are found; the eczema is merely the local expression of a constitutional vice: the cause is constitutional; why the lesion is cutaneous we do not know. To treat eczema successfully we must not concentrate our attention and our remedies on the eczema only; but, while adopting suitable measures for soothing and healing the inflamed skin, we must carefully study and favourably modify the diathetic condition which underlies the local mischief, and is its essential constituent.

The writings of Dr. Tilbury Fox, who brought to the study of dermatology not the narrow vision of the specialist but the broad insight of the cultivated clinical physician, are characterised by an enlightened recognition of the *rôle* of constitutional states in the causation of diseases of the skin; he did more, probably, than any recent writer to expound and explain the dependence of cutaneous disorders upon general abnormalities.

The most prominent and best recognised of the causal relationships of eczema are those it owes to struma and to gout. The eczema of infants, children, and young adolescents, especially when the discharge from the affected surface contains pus, is nearly always strumous, and yields only to cod-liver oil, iodides, iron, and suitable hygienic measures. Fox went further; he taught that "senile struma is an important state to recognise." On the other hand, the chronic eczema of middle and advanced life, especially in males, is commonly gouty, and is benefited by purgative mineral waters, Friedrichshall, Hunyadi Janos, &c., colchicum, iodide of potassium, alkalies, and lithia, and an arrangement of diet and exercise calculated to promote the excretion of uric acid. The nervous temperament often seems to have some pathological relationship with the gouty diathesis; we may see a gouty eczema suddenly aggravated by mental anxiety. Apart from gout and struma, a condition of "general debility," of nervous depression and general feebleness arising from worry or over-work, is often the chief factor in the production of eczema. Here iron, arsenic, rest and good food are needed. A troublesome form of chronic eczema of the legs is apt to arise in connection with chronic renal œdema of the same parts; in this association there is often a gouty element. Chronic eczema may have a syphilitic basis and yield to specific treatment. Eczema does not occur as a syphiloderm; but the chronicity of an eczema is often due to an unexhausted syphilitic taint, either acquired or hereditary. As illustrations of the dependence of eczema upon general constitutional states, I have only pointed out some of the more prominent etiological alliances of the malady; I might mention others, such as "sluggish liver," and gastric and bronchial catarrh.

One of the best local remedies for eczema is the ointment of oleate of zinc, for which the profession is indebted to Dr. Crocker. After using the preparation for several months, in a large number of cases both in hospital and in private practice, I have elsewhere ventured to express my testimony in its favour. Some time ago, with the view of testing its action in the treatment of eczema, I desired Messrs. Southall of Birmingham to make for me an ointment of oleate of lead. After a series of experiments they produced an excellent ointment according to the following formula:—Lead oleate, 24 parts; heavy and inodorous paraffin oil, 14 parts.

The lead oleate is prepared by heating a mixture of oleic acid and oxide of lead. I can confidently recommend this ointment as a very efficient local application in eczema. I have used it successfully in a large number of cases.—*Practitioner, Nov.* 1879, *p.* 346.

81.—THE DIAGNOSIS AND TREATMENT OF PSORIASIS.

By Professor MAYNARD, Chicago.

In a lecture delivered at the Woman's Medical College, Chicago (New York Med. Record, Dec. 13), Prof. Maynard having before him a case of psoriasis exhibiting admirably the different phases of the affection, drew the attention of his class to some of the points of importance in its diagnosis. The disease always commences with small reddish spots that are immediately covered over with whitish scales; and accordingly as these spots assume different forms they have received different names—as psoriasis guttata, nummularis, diffusa, &c. By scraping the pearly or whitish scales which cover these patches, and which are very easily detached, we obtain a view of the bright red or hyperæmic base, upon which a number of minute bleeding points have sprung up. This is so characteristic an appearance of the disease that it can always be relied upon as an aid to diagnosis when there is any doubt. The eruption, too, is usually found most diffused and best defined on the extensor surfaces of the limbs, so that the elbows and knees are often the only parts attacked. The eruption of psoriasis also is always dry, no exudation existing as in eczema. There is more or less itching, but it is at times so slight as not to cause any discomfort. Occasionally, psoriasis, which is usually easily recognised, assumes in its later stage an appearance closely resembling ringworm. In this, however, the scales are not so thick and bulky as in psoriasis, and are more branny, and of a greyish colour. In ringworm, too, there is no tendency to symmetry in the eruption, and there is a history of contagion; the microscope also detects the mycelium and spores of a vegetable parasite, which never exists in psoriasis. Sometimes there are found chronic patches of eczema that have much resemblance to those of psoriasis; but on examination it will be found that the itching from them is much more intense and constant; that in most cases there has at some time been exudation; that the scales are much more firm and tenacious; and that the borders of the patches are not so well defined as in psoriasis, having a tendency to shade away into the surrounding skin. Occasionally, syphilitic psoriasis bears so close a resemblance to the non-specific form that much care and experience is required for the diagnosis, which is of great importance as regards treatment. In the syphilitic form, the spots instead of being of a bright red are of a dark red or purplish colour, and only covered partially with thin branny scales, which, on account of their tendency to rapidly exfoliate, leave the base quite exposed. In this variety the elbows and knees are not necessarily affected, the eruption occurring

on any part of the body without regard to symmetry in development. Patches of psoriasis that exist solely upon the palms of the hands are, in Prof. Maynard's opinion, invariably syphilitic.

Although strongly disposed, when treating skin diseases, to search out for internal conditions which may have acted as causative or modifying influences, Prof. Maynard states that, with respect to psoriasis, he has generally failed in detecting these; and he is no believer in its strumous, dartrous, or gouty origin, advocated by some theorists. He is of opinion that to a certain extent the skin possesses a sufficient amount of inherent independence to carry on special pathological changes without the co-existence of other functional disturbances, and that there are also diseases of the skin which are essentially local in their nature and are cured by local treatment. Psoriasis may be regarded as one of these, being a simple hyperæmia of certain portions of the skin, including the upper layers of the corium, and accompanied by a manifest proliferation of dried epithelial cells. This being so, there is no indication for internal treatment, local remedies sufficing in many cases. Still, Dr. Maynard very frequently prescribes arsenic, believing it to be an excellent adjunct to external remedies. He does not agree with the general opinion that arsenic is a sovereign remedy in skin diseases, in few of which, indeed, is it very beneficial. It is given in psoriasis as a simple tonic, which, during its elimination through the skin, plays the part of a cutaneous irritant, stimulating the process of repair. That it acts as a stimulant to the rete, and even as an irritant to these cutaneous cells, is shown by the fact that acute psoriasis, or other acute disease of the skin, is invariably made worse by its employment, while the more sluggish and indolent the disease, the more rapid is the improvement under its use. It should always be begun with in small doses—generally three or four minims of Fowler's solution diluted in water—giving it quite regularly three times a day after meals. The arsenic may have to be continued perhaps for months, and it is rarely necessary to increase the dose, which indeed, small as it is, may give rise to swelling and itching of the eyelids, pains in the head, gastric disturbance, &c. When this is the case, the dose must be diminished or the medicine suspended. As to local treatment, where the eruption is of recent date, and running an acute course, soothing applications must be used. In these cases there is a constant exfoliation of scales which must be first removed by plain or alkaline baths, a very good one being made by dissolving a pound each of borax and bicarbonate of soda in thirty gallons of water. After the bath the patient should be anointed with cosmoline or vaseline, and this treat-

ment should be continued until the eruption fades away or becomes sufficiently chronic to allow more stimulant applications.

In the chronic stage, Prof. Maynard thinks highly of chrysophanic acid, fifteen grains to the ounce of lard. It should be applied with great care only to the patches of the eruption, as in some it produces great irritation of the skin. An objection to its use is the peculiar staining of the skin which it produces; and on this account it is best to employ it on parts covered by the clothing, substituting for it on the face and hands the sapo viridis, or German green soap, which may be used alone, or, if found too irritating, with one or two parts of alcohol. Pyrogallic acid (in a 10 per cent. solution in water) has been recommended as a substitute when the chrysophanic acid cannot be tolerated, being much slower and milder in its action. Liquid pitch, or oleum cadini, may also be used with good effect, either as ointment with lard or cosmoline, or in lotions with alcohol or eau-de-cologne. Usually the disease will yield to these means, but occasionally it proves very obstinate and very liable to relapse, especially in winter. In such cases the arsenic treatment should be continued for several months after the subsidence of the eruption. "From these personal investigations, then, I am led to the belief that psoriasis, in a majority of cases, is purely a local disease, that it is often removed by local means alone, and that arsenic is curative only as it acts in conjunction with the local treatment. I am a warm advocate of the influence of functional disturbances in their relation to skin diseases. . . . While claiming the local origin of psoriasis, it is but just to say that my opinions are not in accordance with those of many dermatologists, and that a majority, perhaps, will favour its constitutional origin."—*Medical Times and Gazette, March* 27, 1880, *p.* 354.

82.—ON THE PECULIARITIES OF RINGWORM AND ITS TREATMENT.

By Dr. ROBERT LIVEING, F.R.C.P., Lecturer on Diseases of the Skin to the Middlesex Hospital.

Our most neglected class of children—"gutter children," as they are called—are practically exempt from tinea tonsurans. The chief reason of this is that they do not attend school and do not, as a rule, associate with school children, and perhaps partly because they do not use brushes. This fact is at all events sufficient to explode the old idea that ringworm has anything whatever to do with want of cleanliness. It also leads me to call your attention to the way in which the disease is propagated—namely, by schools. They are, so to speak, the

nurseries of ringworm. When it appears in a family it is almost always introduced by the child who goes to school. One of the most striking peculiarities of this affection is the influence that age exercises upon it. We rarely meet with ringworm of the scalp in an adult, and when it attacks the body, it often disappears again without treatment—in short, it dies a natural death. And what is still more remarkable is that even inveterate cases of the disease, that have defied all treatment, usually get well as soon as the child reaches the age of fifteen or sixteen. There are, of course, now and then exceptions that prove the rule. One might be tempted to suppose that vegetable parasites have a predilection for young and tender skin, but this, at all events, does not apply to the parasite of pityriasis versicolor, which is essentially an affection of adult life. Again, infants at the breast often get ringworm, but I have never met with a case in which the eruption has given much trouble; the mildest remedies cure it. No doubt the imperfect development of hair may account for this fact.

Let me next direct your attention to some peculiarities in the *appearance* of tinea tonsurans. Generally, nothing is easier than the diagnosis of this affection. One or two scurfy, itching spots on the head, of somewhat rounded form and studded with opaque, broken, and twisted hairs, leave no doubt as to the nature of the disease. But the cases that come before us are not always of this typical kind, and that the diagnosis is somewhat difficult is proved by the fact that a difference of opinion often exists as to the nature of a particular case. Everyone knows, for example, that it is a really difficult matter to differentiate between tinea tonsurans and favus in certain stages of the latter disease; but as you will very rarely be called upon to treat favus in this country, I shall dismiss that part of the subject. You are well aware of the fact that ringworm of the scalp presents a different appearance from ringworm of the trunk. If you observe a large number of cases of ringworm, you will find a certain proportion which have, in addition to the eruption on the scalp, a few small, red, scurfy spots on the face, neck, or other parts of the body; you have little difficulty in determining that these red spots are produced by tinea tonsurans; but supposing similar spots occur on a child who has no ringworm of the head, and you have no history of the disease in the family, you may then have great doubt as to their nature, for they closely resemble little spots of so-called simple pityriasis or dry eczema. I would remark, however, that the ringworm spot is usually of a brighter colour, and has a more abruptly defined margin; if one of the spots is of a rounded form, with a narrow, bright, slightly raised edge, that will of course greatly help your diagnosis. A *solitary*, round,

scaly spot, even without a distinct margin, should always make you suspect tinea tonsurans, and induce you to make a careful examination. Returning to ringworm of the scalp, which is far more important than that of the body, we notice that there are several forms or stages in which the diagnosis is a little difficult. (1) In a very early stage, before the hair is visibly affected, we find only red, scurfy little spots, which may be very easily overlooked or mistaken for some other affection. As this is an important stage of this disease with reference to the treatment, it is necessary to be on the look-out for it. The hair will generally be found to be abnormally brittle over these spots, although to the eye it may appear natural. (2) We now and then meet with chronic cases in which the hair grows freely over the head, no patches of any kind are found, and the skin looks generally healthy; but a careful examination with a magnifying glass detects isolated or small groups of hairs, scattered here and there over the head, presenting all the characters of tinea tonsurans. Dr. Alder Smith, who has had great experience in this affection, tells me that these are the cases most frequently overlooked by medical men, and supposed to be cured when they really are not. This is hardly to be wondered at when we consider how difficult it is to find isolated hairs; indeed, if you do not know beforehand that the child has suffered from tinea, there is nothing to lead you to a correct diagnosis. (3) Tinea tonsurans is easily masked by attendant eczema, especially when the crusts are thick; under these circumstances a careful examination of some of the hairs is necessary. *A shedding of the hair in patches*, in a case of eczema, should always make you look out for tinea tonsurans. (4) I have occasionally pointed out to you examples of ordinary ringworm which lead to *perfectly smooth bald patches*, not to be distinguished by the naked eye from alopecia areata; indeed, if you have not the opportunity of watching these cases as they pass through their early stages, it is almost impossible to say whether you have before you area or tinea tonsurans; all the diseased hairs have, in fact, been shed. I have on several occasions met with an instance of this in a family affected with tinea tonsurans, the other cases being of the ordinary form. As I have elsewhere pointed out, it is this variation from the usual course that has given rise to the erroneous idea that there exists a disease—*tinea decalvans*—distinct from tinea tonsurans on the one hand, and from alopecia areata on the other. (5) You may possibly mistake some few cases of alopecia for tinea tonsurans. Under ordinary circumstances it is very easy to distinguish between the two. In area the hair comes off freely and quickly, leaving a clean bald patch; moreover, it occurs especially at the occiput and behind the ears, and the eyebrows

are often affected; but now and then the hair is not shed freely, and we find partially bald, round patches, covered pretty thickly with short, stumpy and broken hairs, having a close resemblance to common ringworm. A little care and the microscope will, however, enable you to avoid an erroneous diagnosis. We have now under observation, as an out-patient, an example of this in a young man in whom the hair is much thinned and broken over nearly the whole of the scalp, but which is nowhere quite bald, and if he were not losing his eyebrows and eyelashes, we might have some difficulty in recognising it as a case of alopecia areata, though in this particular instance the age of the patient and the history of the attack would preclude the possibility of its being common ringworm.

There is a very important question which you will be constantly asked, and that is as to the communicable nature of ringworm, and at what period it ceases to be contagious. Now, strictly speaking, it is so until it is quite cured; indeed, as long as scurfy spots remain, even though the hair be growing freely, it may be propagated. It is, however, far more contagious when it first appears, before any remedies have been applied, than it is while under treatment. The best remedy to prevent its spreading amongst children, when they are obliged to associate together, is the carbolised glycerine of the British Pharmacopœia, either pure or diluted with a little more glycerine; it should be well rubbed all over the scalp every morning. This acts in two ways: (1) The carbolic acid produces its usual effect on organised matter; and (2) the glycerine prevents particles of scurf from being dispersed. Of course, the chief objection to this treatment is that the smell of the carbolic acid is very disagreeable to some people; on the other hand, it is sometimes impossible to separate children entirely, and any means of preventing the propagation of the disease is valuable. In addition to this, the carbolised glycerine has a distinct curative effect.

And now as to the *treatment* of this troublesome malady. Nothing is easier to cure than tinea tonsurans of the trunk, or more difficult to deal with than the same disease when it is well established on the scalp. It is important that you should understand how the remedies in common use act. They may be conveniently divided into two classes—(1) Those which act by setting up sufficient inflammation in the skin to lead to the destruction of the disease; (2) those of a milder kind, which act simply as antagonistic to the development of the Trichophyton tonsurans. To the former class belong such remedies as acetum cantharidis and strong acetic acid; to the latter belong sulphur ointment, the white precipitate ointment, and sulphurous acid lotion. Many remedies combine, as it were,

these two properties; as, for example, chrysophanic acid ointment, iodine liniment, and strong carbolised glycerine. How are you to choose between all these and many other remedies? You must be guided by circumstances, and take into consideration both the age of your patient and also the extent of the mischief. *Strong remedies are always contra-indicated in very young children;* a little tincture of iodine painted on once a day for a few days, followed by the use of the white precipitate ointment, is all that it is necessary. In older children stronger treatment must be used, but even then you must be guided in your choice by the extent of the mischief. It is very unwise to make a *large* sore place on the scalp, as it will very likely give you and your patient more trouble than the ringworm itself. If, however, the disease is in an early stage, and consists of one or two small circumscribed spots, your best plan is to cut the hair short all round the spots, and apply with a brush Coster's paste, acetum cantharidis, or iodine liniment. At this stage a few applications will sometimes arrest the mischief. A single painting with pure carbolic acid is thoroughly effective, but it is a strong remedy, and gives some pain. Always bear in mind that it is very unwise to trust strong remedies to unskilled hands. When the disease extends over a large surface you must be content with using milder measures —tincture of iodine of double strength painted on every day is a good and safe mode of treatment. This may be followed up by the use of the nitrate of mercury ointment, diluted according to circumstances, or an ointment containing the red and white precipitate of mercury and sulphur, or the oleate of mercury (10 per cent.) For many years I have used in certain cases goa powder or chrysophanic acid ointment (thirty grains to the ounce is usually strong enough), and I have found it a very effective remedy, but there are great drawbacks to its general use. First, it stains everything with which it comes in contact, and in the second place we are uncertain as to the amount of inflammation it may set up; some children bear it well, while in others it produces so much irritation, swelling, and discolouration of the skin as to alarm those who use it. It must therefore be used with caution, and patients should be warned of its properties; nevertheless, I repeat, it is a very effective remedy.—*Lancet, Nov.* 1, 1879, *p.* 642.

83.—ON SOME POINTS CONNECTED WITH THE DIAGNOSIS AND TREATMENT OF SCABIES.

By Dr. ROBERT LIVEING, F.R.C.P., &c.

In order to avoid mistakes, it is well to know the circumstances under which this disease is generally contracted. Every

medical man knows that it is most frequently introduced into families by one of the servants, and in any suspected case he always very properly examines all the servants; but if he fail to find any scabies amongst them, he is generally unable to account for its appearance. Now, I wish to point out a fact which I have noted for a long time past, and that is, that every autumn I have an unusual accession of patients suffering from scabies, and that these patients have usually been travelling on the Continent. I am now quite satisfied that the disease is not unfrequently caught in this way. I am not able to offer any information in detail as to how it is contracted abroad, but of the fact I am quite sure.

There are certain mistakes often made with regard to the *diagnosis* and *treatment* of scabies, which I find repeated with great regularity, and which I propose to point out. With regard to the diagnosis, errors often occur under the following circumstances. 1. A person, we will suppose, has suffered from time to time from attacks of eczema of the hands or elsewhere, and his medical adviser knows the fact; this person contracts scabies; what can be more natural than that his medical attendant should overlook it? The patient himself believes that he has, perhaps, a more obstinate attack of eczema than usual, but nothing more, and he is at first not surprised that he does not get well under the usual treatment. 2. In children, scabies in an early stage is often masked by a copious eruption of urticaria, and attention is naturally drawn to the more striking features of the eruption, the cause being overlooked; this mistake may be made by anyone, even the most careful, but it is generally soon corrected. 3. It is not very uncommon to meet with well-marked cases of scabies in which the hands and wrists are free from eruption; and, if the medical man be not aware of this, he is apt to be misled by it. I attribute this occasional freedom of the hands from eruption to the present very common use of more or less medicated soaps. In nearly all the instances of the kind that I have met with, I have been able to trace the cause to this source. In the case of hospital patients, the same results have been produced by certain occupations. In all doubtful cases of scabies in males, the *lower part of the abdomen, buttocks, and penis* should be examined; the appearance on the skin of the penis is very characteristic. In women, where a thorough examination is more difficult, we should especially look out for eruption about the mammæ and axillæ. In young children, the hands sometimes escape for a time, while the feet and buttocks are early attacked. 4. A very common error occurs in the following way. A patient suffering from an eruption consults a medical man, who believes it to be the itch, and orders accordingly the sulphur ointment

of the *Pharmacopœia* to be well rubbed in; the patient, wishing to get rid of his trouble as fast as possible, carries out the treatment very vigorously; he is probably much better after the first and second application, but he soon begins to get, as he thinks, worse; the eruption extends and becomes more copious, and the more he applies the ointment the more he suffers; after a time, he returns to the medical man, who finds him covered with an irritable eczematous eruption; and, as he now fails to find anything distinctive of itch, he is apt to think that he has made a mistake. But the fact is that he has probably made no mistake, except that of prescribing a very strong ointment, which his patient has used too freely, and thus, while he has cured the scabies, he has produced a great amount of artificial eczema by the process of cure.

With regard to the *treatment* of scabies, errors sometimes occur. The one which is by far the most common, I have already indicated above, namely, that of using a sulphur ointment too strong, and of continuing its use too long. Of all remedies, not one is so effective as sulphur ointment properly applied. An ointment half the strength of that of the *British Pharmacopœia* is quite strong enough, and the best time to use it is at night, when it should be rubbed all over the body, except the head, but especially on the hands, buttocks, and lower part of the abdomen, and then the underclothing used during the previous day, namely, socks, gloves, drawers, and jersey should be worn during the night; this thoroughly disinfects the clothes, and at the same time keeps the ointment well applied to the skin. In the morning, a warm bath may be taken, and no treatment followed during the day. For three nights the process should be repeated, but never longer; subsequently a little ointment should be well rubbed on the hands, wrists, and buttocks for a few nights. All treatment should then be discontinued for at least a week, when, if necessary, it may be repeated for one or two nights, or a milder ointment might be used. It is sometimes difficult to say whether a case of scabies is cured or not; under these circumstances it is very convenient to use an ointment which does not irritate or annoy the patient by its disagreeable smell, and which at the same time will complete the cure. A most excellent ointment of this kind is made with balsam of Peru (℥ij ad ℥j). The styrax ointment is also thoroughly effective, but less agreeable.

With regard to sulphur baths, I would say that they are not nearly as effective as sulphur ointments. I lately ordered sulphur baths (as being more agreeable than ointment) for a pupil of my own who was suffering from scabies; he took six or seven, and then came to me much better, but not cured; I advised more baths, but did not see him again. He had in all

about fifteen baths, and then went home to the country thinking himself cured; unfortunately he was not, and he conveyed scabies to his family. This is not the first time that I have found sulphur baths fail. They are, however, useful under certain circumstances; it may, for example, be very inconvenient to apply sulphur ointment at night. Again, in cases where there is much secondary eczema set up with extensive excoriations, the application of sulphur ointment is very irritating. Under these circumstances, it is very useful to begin with a few baths, which generally produce an excellent effect; this may be followed up by the application of ointment to those regions known to be specially affected.

Lastly, with regard to disinfecting outer clothes and bedding, it can be easily done by sulphur fumigation or baking. In all cases of long standing, the clothes and blankets, &c., should be disinfected, but it is never necessary to extend this to the bed itself.—*British Medical Journal, Oct.* 25, 1879, *p.* 650.

84.—AN IMPROVEMENT IN THE TREATMENT OF PORT-WINE MARK BY LINEAR SCARIFICATION.

By BALMANNO SQUIRE, M.B., London.

The obliteration of port-wine mark without scar (namely, by means of linear scarification), as first recommended by myself two or three years ago, has since become generally practised both in this country and abroad. Some expressions of dissatisfaction with this method as applied to port-wine mark have been published recently in this Journal. A visit to the Continent this summer has shown me that the objections expressed in this country have been felt equally by dermatologists in France, Italy, and Austria. I accordingly felt it a duty to improve my process if possible.

The chief disadvantage of my previous method was the multiplicity of sittings it required; but I have now arrived at reducing the process to two sittings. The obliteration is obtained by the same process that I originally advocated, with only a slight but important difference. The modification in the original process consists in giving the minute incisions an oblique instead of a vertical direction, making them at an angle of forty-five degrees to the surface.

In order to explain why so slight an alteration of the direction of the cuts should lead to so important an abbreviation of the process, it is necessary that I should advert to the view taken by one of my objectors. His objection is, that I do not really obtain by means of linear scarification a definite obliteration of the minute blood-vessels of a port-wine mark when I have divided by means of my minute cuts. I have still

reason to feel satisfied that my original method produced complete obliteration of the minute blood-vessels; but there was one remaining obstacle of which I had not taken account, and that was the as yet unobliterated supply of blood from the venous network permeating the subcutaneous cellular tissue.

I had by my original method of vertical incisions cut off the *horizontal* supply of blood quite finally, by means of my crossed and closely spaced minute vertical incisions, which left no scar. But these vertical squares had to be crossed obliquely by other squares, so often repeated as to tire the patience of many experimenters. Indeed, by such a process, the skin demanded to be traversed by a series of vertical scars which, although so fine as to leave no trace of scar, required to be so closely spaced as to leave the skin almost as finely sliced in a vertical direction as is needed for the production of microscope sections. In short, in my first process, the efficacy of linear scarification remained considerably retarded by the constant free communication of the subcutaneous blood-vessels with the erectile plexus of blood-vessels in the skin. By means of oblique incisions, this underneath supply of blood, also, is at once cut off; and the result is a very speedy and complete obliteration of the port-wine mark.

The details of my new operation are simple. At the first operation, I divide the skin into small squares by two series of parallel incisions, the one set crossed at right angles to the other, just as in my former method; but the incisions, instead of entering the skin perpendicularly, enter it slantingly. I may call the one series of parallel incisions longitudes, and the other cross series latitudes. At my first operation, the obliquity of the longitudinal incisions is directed eastwards, and that of the latitudinal one southwards. At the second operation, executed about a week afterwards, the lines of longitude and latitude are re-executed still in the same directions respectively, only that now the obliquity of the longitudinal incisions is directed westward, while that of the latitudinal ones is directed northward. In this way, the obliteration of a port-wine mark may be effected in a fortnight if the whole of the mark be operated on at once, although it may often be more convenient to treat the mark piecemeal. There are various little details special to this modification of the operation which demand attention; but the above are the main features of the new process.—*British Medical Journal, Nov.* 8, 1879, *p.* 732.

85.—ON THE TREATMENT OF BALDNESS.

In the Atalanta Medical and Surgical Journal, Dr. Geo. H. Rohe states that having been himself a sufferer from seborrhœa

and consequent alopecia for six or seven years, he had tried, but with no success, a great many remedies before he read Kaposi's article on alopecia, in Hebra's Treatise on the Skin. He at once adopted Kaposi's treatment, with the result of checking the fall of hair and diminishing the production of scales in a reasonably short space of time. He has since then recommended the plan in a considerable number of instances, and, when it has been faithfully carried out, with uniform success. The active agent employed is the German or French soft soap (green soap, Schmierseife, savon vert) in alcoholic solution, which, while it is slightly stimulant, removes the scales and thoroughly cleanses the scalp. The following is the formula for the lotion:—℞. Saponis viridis (Germ.), alcoholis āā. ʒii. Solve, filtra, et adde ol. lavandulæ gtt. xx-xxx. This makes a very handsome orange or wine-coloured preparation, with a pleasant odour. It is used as a shampoo every morning or evening, pouring one or two tablespoonfuls on the head. On the addition of water, and smart friction with the fingers, a copious lather is soon produced. After keeping up the shampooing process for four or five minutes all the soap must be washed out of the hair by the free use of warm or cold water, and the hair thoroughly dried with a soft towel. The immediate effect experienced is a disagreeable feeling of tension of the scalp, as if it were stretched too tightly over the skull; this is relieved, and the scalp prevented from becoming too dry, by some fatty application, the best, as well as the neatest preparation which can be used for this purpose being cosmoline, a hydro-carbon obtained from petroleum. Cosmoline is perfectly bland and unirritating, never turns rancid, and is comparatively cheap, it may be obtained in the fluid form or as a soft solid. This procedure must be repeated daily for three or four weeks, when it will generally be found that the production of scales and the falling of the hair have been very markedly decreased. It will then suffice to use the preparation two or three times a week for a month or two longer, after which a good shampoo once a week will usually succeed in maintaining a permanent cure. It is not necessary, though it is more convenient, to cut the hair short during the treatment. Patients should be warned that on beginning to use this method the hair sometimes comes out in greater quantity than before, as many of the hairs are dead and retained in their follicles only by the plugging of their sheath with the accumulated sebaceous matter. Nothing will restore the hair on the affected parts if the alopecia has lasted so long that the hair-bulbs have become atrophied; in such cases our endeavours should be directed to saving what remains. A prognosis favourable to the restoration of the hair must therefore be given with caution.—*Glasgow Medical Journal, Nov.* 1879, *p.* 391.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

86.—ON DIGITAL DILATATION OF THE OS IN LABOUR.

By Dr. JAMES BRAITHWAITE, Lecturer on Diseases of Women and Children at Leeds School of Medicine; Surgeon to the Hospital for Women and Children.

The first stage of labour is often unduly prolonged, the amount of dilatation effected being quite out of proportion to the length of time consumed, so that the patient becomes wearied and exhausted before the real effort is required. This may arise from various causes—causes so familiar that it is unnecessary to dwell upon them. There are also occasions in which a rapid dilatation of the os uteri is desirable or necessary—cases of severe puerperal convulsions, occurring simultaneously with the commencement of labour, and requiring as rapid a completion of labour as possible to save the patient's life.

Our means of assisting dilatation in such cases are most unsatisfactory. Barnes's dilators, valuable though they are in many cases, are not always suitable, and digital dilatation as ordinarily practised is of but little use, and indeed is not suitable for the period of labour in question. Two years ago I was detained a day and night in a country house by a case in which the membranes having been long ruptured the thick and fleshy os would not dilate, or did so so slowly and imperfectly that the patient's strength became at last exhausted. There was a little narrowing of the brim, and with each pain the head descended, covered to a great extent by the os and impinged upon the bones. With the fingers of one hand I tried to assist dilatation, but almost in vain. After the lapse of some hours I delivered her with the greatest difficulty by means of the long forceps passed within the os. The child was living but died the same day, owing to a bulging inwards of a portion of one parietal bone, the size and shape of half a small watch glass.

I blamed myself very much at the time for this occurrence, as no doubt it was caused by the forceps, though I think not by the point of a blade, as they had taken good hold and had not slipped. I made a small incision at the edge of the depression and restored the bone to its place by raising it with the small blade of a new and sharp pair of scissors. The wound

bled very freely, but the bleeding was arrested by pressure, though not entirely, for a little oozing occasionally occurred, and owing to the feeble state of the child from prolonged labour sufficed to turn the scale against it.

This occurrence was naturally very painful to me, for though I think I may say I have attended hundreds of forceps cases in my own practice and in those of other medical men, I never met with anything of the kind either before or since. The difficulty in this case was entirely caused by the slow dilatation of the external os, and if I had possessed any means of dilating it I believe the child's life would have been saved. I may say the mother recovered without a bad symptom. I had Barnes's dilators with me, but did not use them, having previously found that in such cases they are inapplicable, owing to the impossibility of getting a sufficient portion of the bag within the os and retaining it there. The only other instrument likely to be of any use would have been the ingenious dilator of Mr. Herbert Morgan, of Lichfield. This is a conical india-rubber bag, the shape in section of an equilateral triangle. The flat top is stiffened by canvas, though not so as to prevent its being folded for the purpose of introduction, but sufficient on expansion within the os to cause it to resist withdrawal when traction is made upon the tube attached to the opposite angle. The bag having been expanded with air or water precisely imitates the natural mode of dilatation by the membranes.

I believe this to be a valuable instrument, but like other valuable things it is not always at hand when wanted.

The mode of using the fingers as a dilator, which it is the object of this paper to explain, and which was originated by reflecting upon the case related, differs from any of the methods of digital dilatation in that both hands are brought into play at the same time. Probably my meaning will be better understood, and certainly its value, if it possess any, be more fully appreciated by a relation of cases rather than by a formal explanation.

Mrs. P., primipara, eight and half months pregnant, blonde, complexion almost colourless, legs œdematous. She was suddenly roused from sleep in the middle of the night by the loud noise of a passing fire-engine. She immediately became convulsed, and went on from bad to worse, until by noon next day death was imminent. There were at this time no intervals of consciousness, and one convulsion followed another so rapidly that the face never lost the livid hue produced by the laryngeal spasm. Chloroform could not be given, as it seemed to increase the spasmodic action of the laryngeal muscles, and its vapour did not reach the lungs in amount sufficient to cause cessation

of convulsion. Bleeding was not attempted on account of the bloodless appearance of the patient. I found that the os was dilating, and being very thin and extensible I introduced first the right index finger within it, and then the palmar surface of this finger being towards the sacrum of the patient, I passed the left index along the back of the right with its palmar surface towards the pubis, the left hand passing over the right for this purpose. The tip of the left index was then insinuated within the os, and the two fingers laid exactly back to back. By slight separation of the terminal phalanges I could now dilate the os at pleasure. It will be noticed that the dilating pressure thus made is not a direct outwards one against the edge of the os, and which would crush and injure it, but it is downwards and outwards, for the finger tips are hooked within the os, and the dilatation thus very closely resembles the natural one.

It is, moreover, made by a soft substance, and any bruise or injury of the parts is the fault of the operator, for his instrument is sensitive, and enables him to gauge accurately the amount of dilating force advisable. The amount of force thus at command is, of course, much in excess of what is required, but this is no argument against a moderate degree of pressure in such cases as this, conjoined with extreme care and gentleness. The only objection which I can see to this mode of dilatation and to similar ones which I have still to explain, is that the pressure is not circular; but it is easy, by a slight change in the position of the fingers, to vary the direction of the pressure, so that the whole circumference of the part to be dilated is more or less acted on. During this time the patient was in constant and sometimes violent movement; when the movements were more than usual the fingers were withdrawn, but there was no time to lose, as her death was momentarily expected. I was surprised at the rapidity and ease with which the os expanded. The dilating process was not used continuously but intermittingly, so as to resemble the natural pains. When the os became as large as a five-shilling piece, I began to dilate with three fingers instead of two, the hands then not being crossed, but two fingers of the left hand, having their palmar surfaces pointing towards the sacrum of the patient, and the index of the right, back to back with them, its palmar surface pointing anteriorly. The os soon became more widely dilated, so the first two fingers of each hand were employed in the same relative position as before. It may be supposed that this mode of using the fingers would be awkward to the operator, but this is not so if he possesses ordinary manipulative skill, and his fingers are not unduly large.

In this case the os was very rapidly and easily dilated to the

size of the rim of a large teacup, when the long forceps were applied and the patient delivered of a dead child of small size, evidently not quite at the full time.

The whole process, from commencement of dilatation to delivery of the child, occupied less than an hour.

After delivery the convulsions became separated by more distinct intervals, and the administration of chloroform became practicable, the respiration being sometimes almost natural. The convulsions were kept off by chloroform for twelve hours, when they ceased spontaneously. Recovery gradually followed, no bad symptoms of any kind occurring, and the patient is now pregnant for the second time. With regard to the use of four fingers, *i.e.*, the first two of each hand, I found the advantage to be that the posterior margin of the os could be reached by the left digits, whilst the right at a rather lower level were inserted within the anterior edge. Of course it is obvious that such a process as this can only be defended on account of its being actually found from experience to be not injurious, and I can most sincerely say that in the cases in which I have employed it, all of which, as they are not numerous, I shall relate, in only one has anything at all approaching suspicion of mischief occurred.

Case 2 was the wife of a dissenting minister. Primipara. No œdema of the legs, and near the full term of gestation. I called upon her, as requested by her husband, found her apparently in excellent health but thought her nervous and excitable. The same night she began in puerperal convulsions; they were, however, separated by intervals sufficient to allow of free respiration. She was half unconscious in the intervals between the convulsions and unable to answer any question, but I soon noticed that each fit was synchronous with what appeared to be a labour pain.

Towards noon the next day she began to be in danger, and I then assisted dilation of the os the same as in the last case. The os at this time was the size of a florin and very thin and dilatable, and after it had reached the size of the rim of a small teacup the pains came on regularly and naturally, so that I interfered no further, but confined my attention to giving chloroform. The labour was concluded naturally and the child was living and did well. Chloroform was given for many hours after the labour by the husband of the patient, as it was found easy to stop each fit by a very small amount of chloroform.

The patient recovered well and has been in excellent health up to the present time. The advantage gained by the digital dilatation was a shortening of the labour by several hours, and as after delivery the convulsions became much less severe and

quite controlled by chloroform, I think the risk of the case was much diminished.

Case 3.—Mrs. T. was not in good health before labour commenced of her sixth child. She was very stout and accustomed to take hardly any out-of-door exercise. Pains began on a Friday, probably not true labour pains, for they occasionally passed off for several hours, and real labour did not begin till the following Wednesday. During these five days but little food was taken and she had hardly any sleep. On the Wednesday morning I found the os the size of a shilling; by six o'clock it was only the size of half-a-crown, and the patient was very weary. I ruptured the membranes, thinking excess of liquor amnii might be the cause of the slowness of the case. By eleven o'clock, no further advance worth mentioning having been made, and the pains being very feeble, I dilated a little with three fingers, but found the resistance was a ring of muscular fibre about an inch within the os. This yielded at once to a little gentle pressure, and the os having been a little dilated nothing further was done for half an hour, when more dilatation was effected in aid of the pains, which were now increasing in strength and frequency. The rest of the case was completed naturally and rapidly; there were twins, and of course this partly explained the tediousness of the first stage of labour. Mother and children did well, but finding that five months afterwards she was menstruating freely, and had done so two or three times, I examined and found the uterus enlarged, measuring three and a quarter inches internally, and with one little spot of erosion. This has been successfully treated by ergot internally, Dr. Wynn Williams' solution of iodine locally, and a momentary cold bath every morning, with quinine three times a day. This is the only case in which any uterine affection followed a labour in which I had employed digital dilatation, but it cannot in any degree be traced to this; it was an ordinary subinvolution.

Case 4.—Mrs. F. was in labour of her sixth child; the waters had broken early and the head of the child failed to dilate the soft and very thick os uteri, but with every pain carried it down, and became jammed in the rather narrow inlet of the pelvis. The pains, although good, thus produced little result, so I used the two fingers of each hand to dilate the os, and the dilatation effected was most satisfactory, so that I was enabled to deliver her by the long forceps without much difficulty. Delivery was indeed effected "in a few minutes," as my notes made at the time tell me. It should be noted that this case much resembles the unfortunate one with the relation of which I commenced this paper, but the ease of delivery and the result were very different. This patient has been in excellent health

during the fourteen months which have elapsed since her child was born.

Case 5.—Primipara. Os dilated very slowly in spite of the frequency of the pains, which were indeed too rapid and too strong for the early stage of labour. I should in such a case have ordinarily given chloroform for a few minutes, but instead tried digital dilatation. 11 a.m., os was the size of a florin, hard and unyielding, pains strong and recurring every three minutes. Dilated with two index fingers very carefully and gently every quarter of an hour; continued this until 1.30, using when necessary three fingers. After this I interfered no further. By 2 p.m. the os was fully dilated and membranes unruptured; labour concluded safely at 3. The child was brought to my house by its grandmother at the age of three months, and she reported her daughter as being in good health.

These are the cases in which I have employed this method of dilatation; they have all occurred during the last two years, during which I have, partly by the aid of an excellent assistant, attended more than four hundred cases, so that it will be seen it has not been employed needlessly or simply for the purpose of experiment.

It may be expected that if this plan be attempted in cases which do not require it and only to save the time of the accoucheur, that the patient would complain of the interference, but in cases where the patient is herself conscious of needing some assistance from the length of the case or from a feeling of exhaustion, any assistance will be welcome. Of course the utmost gentleness and care are necessary, and a good deal of the manipulative dexterity so necessary in our branch of the profession, but with these, and by choosing only those cases really requiring aid of this kind, I am convinced that only good will result.

Whether my plan is new or not I do not know, but I am unable to find it referred to by any writer on midwifery, and I judge from its not being mentioned in a very able and interesting article with the same title as mine, by Dr. Stevenson, of Aberdeen, published in the Obstetrical Journal for August, that if it is not new, it is at least not known.

I have very little doubt, however, that it has occurred to some member of this Society, and I know one gentleman, Mr. Ford, of Harrogate, who tells me he has employed it. My feeling is that it supplies a want in midwifery and had it occurred to me earlier my little patient with the depressed parietal bone would probably have been living at the present time.—*Obstetrical Transactions*, vol. xxi, 1880, *p.* 38.

87.—TWELVE CASES OF DILATATION OF THE CERVIX BY CONTINUOUS ELASTIC PRESSURE.

By LAWSON TAIT, Esq., F.R.C.S., Surgeon to the Birmingham and Midland Hospital for Women.

I have had such unsatisfactory results with all kinds of tents in dilating the cervix uteri that I have long desired to get something which would accomplish this more safely, more speedily, and with less pain. Having been struck with the ease with which an inverted uterus can be returned by continuous elastic pressure, I applied this method for the purpose of dilating the cervix; and my results have been so completely satisfactory that I hasten to narrate my experience of the first twelve cases in which I have used it.

Case 1.—E. W., aged twenty-seven, had a miscarriage in Dec., and has been losing more or less ever since. Cervix closed; fundus large. No. 2 dilator was applied at 6 p.m.; No. 3 at 10 p.m.; and next morning at 9.30 I could introduce my finger easily, and removed what seemed to be the whole of a placenta of about the third month.

Case 2.—Mrs. H., aged thirty-eight. Fundus large; cervix small; profuse menorrhagia. Clearly a case of multiple myoma. A trial of No. 1 dilator resulted in partial dilatation after six hours' application. Twelve hours' application of No. 2 produced no effect. I therefore notched the cervix on each side to the depth of a quarter of an inch, and directed the house-surgeon to insert No. 2 once a day for a few minutes. On the third day I applied No. 3 at 7 p.m.; at 10 p.m. No. 4 was placed by the house-surgeon; and at 9 next morning the whole canal was so fully dilated that the finger could reach the fundus.

Case 3.—E. H., aged twenty-eight. Fundus large and completely retroflexed; profuse menorrhagia. The retroflexion made it at first impossible to use the dilators, and I therefore divided the cervix freely on each side by the hysterotome, and placed one of Marion Sims' cleft stems in the cavity. This was worn for about a fortnight. Dilatation was begun about 3 p.m. by No. 3; this was replaced by No. 4 about 10 p.m.; and at 10 next morning the canal was fully dilated, so that I could discover and open the capsule of a small myoma in the fundus, which was afterwards expelled by the uterine contractions.

Case 4.—Mrs. F., aged thirty-nine. Profuse metrorrhagia, probably from retained fragments of a placenta belonging to a miscarriage which occurred seven months ago. No. 2 dilator was applied at 5 p.m.; No. 3 by the house-surgeon at 10 p.m.; and when I visited the hospital next day about 6 p.m. the canal was fully dilated, and I removed the placental *débris*.

Case 5.—Mrs. W., aged thirty-eight, had a miscarriage six

weeks ago, and has been losing ever since. Fundus large and cervix closed. Applied No. 2 dilator at 11 a.m.; No. 3 at 7 p.m.; and at 11 p.m. removed a small placenta.

Case 6.—E. H. has suffered from a continual discharge with severe menorrhagia for more than a year. The apex of a polypus can be discovered presenting through the os. I applied No. 3 dilator at 10 p.m.; and next morning about 9.30 removed a long narrow polypus, the pedicle of which was close to the fundus.

Case 7.—Mrs. S., aged forty-six, has never been pregnant; suffers from continual discharge and irregular floodings. Fundus slightly enlarged; cervix partly open. Applied No. 3 at 7 p.m.; and next morning at 8 could introduce my finger the whole length of the canal. It proved to be a case of endometric epithelioma.

Case 8.—Miss F. H., aged thirty-four, has suffered from metrorrhagia for two years. Has a myoma in the posterior wall. Thinking it might be possible to enucleate it, I applied the dilators. In this case also it was necessary to notch the cervix before they effected their purpose; but after this was done, twelve hours' pressure with Nos. 2 and 3 enabled me to discover that the tumour could not be removed by the cervix.

Case 9.—Mrs. W., aged thirty-seven, has very profuse periods. The fundus is large and retroflexed; the cervix closed. By fifteen hours' pressure with Nos. 2 and 3 I was enabled to introduce my finger, and discovered that it was merely a case of subinvolution. She has now passed two periods since the dilatation with very much less loss, and the retroflexion is practically cured.

Case 10.—Mrs. H., aged twenty-seven, has been losing continuously since her confinement four months ago, and her condition has resisted all therapeutic measures. Twenty hours' dilatation with Nos. 1, 2, and 3 enabled me to remove a small fragment of placenta, not larger than a bean, and since then (nearly a month) there has been no loss at all.

Case 11.—Mrs. M., aged thirty-two, believes she is pregnant about the fourth month, but for six weeks has been losing almost daily. Fundus as large as if she were three months pregnant; cervix slightly open. Eight hours' dilatation with Nos. 3 and 4 enabled me to empty the uterus of an ovum, the placenta of which was situated just within the cervix in the posterior wall of the uterus.

Case 12.—Mrs. H., aged thirty-seven, has been losing at very frequent intervals for seven months, and is never free from an offensive watery discharge. The fundus is large and extremely anteflexed; the cervix quite closed. Eighteen hours' pressure with Nos. 2 and 3 enabled me to remove a rotten fœtus and

placenta, of probably the third month. This ovum must have been retained in the uterus for at least four months after its death, for the last normal period was in Dec., and I removed the ovum on the 1st of August.

Besides these cases I may refer to a case of retained fœtus removed by my colleague, Dr. Savage, after twenty hours' dilatation with Nos. 1, 2, 3, and 4; and, since the above was written, the dilators have been used by myself and my friends in more than twelve other cases with uniform success.

In all of these cases the results were perfectly satisfactory. The advantages of the plan are that it is absolutely free from smell and septic risks, in these matters contrasting most favourably with the use of sponge tents. It is also almost free from pain, and here it has a most incontestable advantage over the use of sea-tangle tents. It is likewise superior to either of these methods of dilating the cervix in that the plugs, being of vulcanite, can be used indefinitely, and that therefore their use is far less costly. I may also say that I have never obtained from sponge, tangle, or tupelo tent the complete dilatation of the whole canal which these plugs produce.

The dilators are best arranged in a set of four sizes made to screw on to a common stem. They are sold to any pattern by Messrs. Mappin and Co., New Street, Birmingham. The only precaution necessary in their employment is to use extremely gentle pressure. I have had a bent stem made for exceptional cases of flexion, but in the great majority of instances the straight stem answers every purpose.—*Lancet, Nov.* 1, 1879, *p.* 648.

88.—STATE OF THE UTERUS IN PLACENTA PRÆVIA.

By Dr. ROBERT BARNES, London.

A clinical observation by Dr. Roper must command attention. His statement that the cervix uteri undergoes "a peculiar induration" in cases of placenta prævia is especially interesting. It is in the highest degree important to dispel the old pertinacious and pernicious error that in these cases "the cervix is always dilated or dilatable." Dr. Roper's observations should help to dispel it.

But I must be permitted to call attention to the fact that in my "Obstetric Operations" I have emphatically pointed out that the cervix in these cases is often rigid and unyielding. The following passage will be found in all the three editions of this work:—So imperious is the dogma of unavoidable persistent hemorrhage, that the difficulty presented by an undilated os uteri is overcome by a special hypothesis, which assumes that in these cases of flooding the os uteri is by the flooding

always rendered easily dilatable. Unfortunately this is not true. Proofs of laceration, of fatal traumatic hemorrhage from the injured cervix, as the penalty of forcing the hand through the presumed dilatable cervix, abound." And to immediate injury must be added the subsequent danger of septicæmia and metritis.

In so far as Dr. Roper's observations of indurated cervix in cases of placenta prævia support the view that this induration is of pathological import, they are, as far as I know, original. But I am inclined to think that the change is physiological in its nature, being pathological only as occurring in an abnormal situation. The gestation is in a physiological sense ectopic. The lower zone of the uterus proper is not so well adapted for the development of the placenta as are the fundal and equatorial zones. But the changes wrought in the placental seat are similar in kind. There is an enormous ectasia of the vessels, and concurrently two conditions ensue. First, some amount of transudation from the vascular channels — anatomically they are channels rather than vessels—into the proper tissues of the uterine wall takes place; some of the transuded serum remains fluid, some condenses or becomes the source of hyperplasia, and thus the uterine wall is not only greatly swollen by the expansion of the vessels by the growth and infiltration of tissues, but it may be hardened by the condensation of effused serum. Secondly, owing to the large proportion of fibrin in the blood, and to the comparative slowness of its movement in the containing reticulated channels, there is a disposition to fibrinous deposits or thromboses in these channels. These changes are observed wheresoever the placenta is attached. They are more marked in the lower zone when the placenta grows there; and these conditions extend to the cervix proper as coming within the range of the exalted action going on in the lower segment of the uterus.

There is a third special cause operating in this region. It is that in many cases—and the remark applies more forcibly to the central implantation cases—the area of growth of the placenta is more limited than when the placenta grows to the fundus or sides of the uterus. The lower zone, even when the placenta grows to it, is of smaller superficies, the placental structure is generally more concentrated, it is smaller in circumference and thicker. Hence there is greater development of uterine vessels at the corresponding site, greater thickness of uterine wall.

Another common cause of rigidity of the cervix in these cases is that, the labour coming on prematurely, the uterus is taken by surprise before its tissues have attained the development necessary for the easy accomplishment of delivery. The child's

small and immature head impinging on the intervening placenta, or often giving place to the shoulder or breech, the natural agent of expansion is wanting.

The lesson to be derived from these anatomical and physiological facts, enforced as it is by clinical experience, is to distrust the old practice of the *accouchement forcé*. It is unscientific, violent—one might say brutal—and dangerous. We must adopt methods more rational and more safe. We must give time for the cervix to expand; remove the obstacle to its expansion which the adhesion of the placenta over the os internum presents by detaching it from this region, expand it gently by water-pressure if need be, not deliver until the parts are fairly dilated; and, if turning be indicated, to accomplish this by the bipolar method, which renders the introduction of the hand unnecessary.—*Lancet, Nov.* 8. 1879, *p.* 708.

89.—ON INTRA-UTERINE TUMOURS.

By Dr. J. MATTHEWS DUNCAN, LL.D., Physician-Accoucheur and Lecturer on Midwifery, at St. Bartholomew's Hospital.

In describing polypi and tumours two things are confused—the origin and the situation of the polypus or tumour; and, like all confusions, this one leads to a great deal of harm. A tumour is best named with reference to its origin. We are now considering only tumours which are intra-uterine in their origin; which spring from the cavity of the body of the uterus, and which remain there. An intra-uterine polypus may be, in point of situation, vulvar—the polypus hanging in the vulva; that is, between the labia. A polypus intra-uterine in origin is, in the majority of cases, a vaginal polypus in situation; or, again, a polypus which grows from the interior of the body of the uterus may be intra-cervical in situation. And when you hear of intra-uterine polypi, or look at pictures or diagrams of them, what is generally meant is intra-cervical. A fibroid or a mucous membrane growth, if truly intra-uterine in situation, is very rarely a polypus. Except in the case of little mucous intra-uterine polypi, I have never seen an intra-uterine growth which was really a polypus.

An intra-uterine growth, not intra-cervical, is either sessile or has only a neck; it has no distinct stalk to make it a polypus. Within the womb proper, there is no room for the development of a stalk to a polypus which is of any considerable dimensions. You must understand, then, that intra-cervical polypi are generally called intra-uterine, and wrongly so; moreover, they are easily diagnosed and managed, compared with truly intra-uterine growths, which are rarely, if ever, polypi, and have only a neck, not a stalk.

I do not attempt to make a new nomenclature; that is an easy proceeding, which is rarely advantageous, and still more rarely successful: but I give a designation to growths which are truly within the cavity of the body of the uterus, calling them intra-uterine tumours, not intra-uterine polypi, from their origin and situation combined; and it is only of such truly intra-uterine tumours that I intend to speak.

You will understand the rationality of calling a tumour or a polypus according to its site of origin, and using other terms to denote the situation in which the body of the growth happens to lie, if you think of polypi of the nose. These frequently hang down into the pharynx, and they are not called pharyngeal, but nasal polypi; and we are only carrying out the same rule of nomenclature.

To recapitulate, truly intra-uterine polypus does not exist, or is never seen except in the case of a small mucous polypus, for there is no room for a stalk to develop to a tumour which attains even a moderate size. The most common uterine tumours are fibroids, and these may be vulvar, vaginal, or intra-cervical polypi, or intra-uterine, or submucous, or embedded, or subperitoneal tumours.

What I have already said indicates that a growth from the interior of the uterus almost invariably grows downwards. It begins within the cavity of the body of the uterus, and as it progresses it becomes, under the influences of growth and uterine contractions, intra-cervical, and then it becomes vaginal, and it may even become vulvar in situation. But this is not invariably the case: a polypus may grow up instead of down; and to prove this I pass round a preparation in which you will observe an intra-uterine growth, almost the size of a small walnut, attached by a neck to the lower part of the cavity of the body of the uterus, and it is growing upwards into the cavity of the body, instead of downwards into the cavity of the cervix. Perhaps if that woman had lived, and we had had a further history of that growth, we might have found that it ultimately descended, following the usual order of things.

One more word before I come to intra-uterine tumours. What are the polypi, intra-uterine in origin, but in situation intra-cervical? They may be polypi of the mucous membrane. Fibrinous polypi are characteristically intra-cervical, though not invariably so. Placental polypi are occasionally intra-cervical, but not generally. Then, there is a rare condition called cervical pregnancy, in which a mole or an otherwise healthy ovum has been pushed, in the process of abortion, out of the cavity of the body of the uterus, its original and natural site, into the cavity of the cervix, but still retaining its connections with the mucous membrane lining the body of the

uterus. Lastly, you have fibroids, either as true polypi, or as spurious or false—that is, partially enucleated.

Now, what are the varieties of intra-uterine tumour? You have three forms of mucous polypi which occur in this situation: firstly, adenomatous, that is, consisting of hypertrophied glandular structures of the uterine mucous membrane; secondly, molluscum, that is, hypertrophy of the areolar tissue without glandular developments; and thirdly, cystic tumours, where the disease is probably the accumulation of fluid within closed glands of the mucous membrane. This cystic degeneration sometimes accompanies or forms an addition to an intra-uterine fibroid. When I do not mention any particular kind of growth you will understand me as speaking of an intra-uterine fibroid. Besides an intra-uterine fibroid you may have a fibrinous polypus within the body of the uterus, or a placental mass. When you have an intra-uterine fibroid it is, as I have already said, a sessile growth, or one which has merely a neck, not a distinct stalk; it is, therefore, not a polypus. It may be a true intra-uterine growth, covered with mucous membrane or with a capsule of muscular tissue in addition; or it may be a spurious or false intra-uterine growth, having no covering, having been to some extent spontaneously enucleated; such a one was at first embedded in the wall of the uterus, and has been expelled, through an opening made in the mucous membrane and muscular tissue, into the uterine cavity, where it may be found as an intra-uterine tumour.

What now are the events which may arise in the history of an intra-uterine fibroid? It may, as in a case I shall presently narrate, cause a woman to bleed till she is nearly at the point of death, and indeed, as I have repeatedly seen, it may prove fatal; or, again, it may give no trouble at all, being found only after death,—not so much as suspected before. It may be pushed down into the cervix; or farther, into the vagina; and perhaps even into the vulva: during which process a stalk is formed, which it did not before possess. It was not a polypus so long as it remained in its place of origin, but when it reached the cervix it became one, whether of the false or true variety; that is, whether still encapsuled or partially enucleated. It may be in the course of this pushing down that it becomes enucleated, or it may be enucleated in its earliest original site, so as to have no covering and lie bare ready to be detached. It may even become completely enucleated; that is, enucleated and detached. Another result still may happen, and is well illustrated by a case which was in "Martha" not long ago. The uterus seizes the intra-uterine tumour as it seizes a mole or a child, and pushes it out; but in the course of this process a stalk is not formed: the tumour pulls the probably thin, and therefore

weak, uterine attachment with it; and consequently you see the woman with an inverted uterus. It is not a polypus which produces this effect; it is a sessile or necked tumour, which refuses, metaphorically speaking, to form a stalk, pulls the womb down, and turns it inside out.

These processes illustrate the polarity of the uterus, of which I spoke in a former lecture. In order that all this may happen you must have an inhibition of the lowest part of the uterine body while the upper segments are working. During the growth of the intra-uterine tumour you had an opposite condition; the lower segment of the uterus was closed and the upper segment expanded. Then comes the polarity change, and the tumour is pushed into the cervix and onwards, forming a stalk, or bringing the inverted organ with it; and all this happens generally with pains and bleeding.

Here is the proper place to tell you an important fact which will enable you to avoid what may be a distressing and serious error. In the course of such a history as we have been describing it may happen that the tumour comes down and again retires. If you examine the woman at one time, most likely while she is losing blood, a tumour will be found in her vagina; but when you return, perhaps intending to operate, there is no tumour to be found—it has gone up again. This occurs not only in the case of polypi and of tumours which are clearly and distinctly within the cavity of the uterus, but also in the case of some which are intramural or embedded in the uterine wall, and are undergoing a process of enucleation and expulsion. I shall endeavour to impress this upon you by the history of a case which came under my own observation some years ago. It was a case of a large tumour in the vagina, which had several times threatened sudden death from loss of blood at the monthly periods, the amount lost being enormous. On examination, I found no tumour at all in the vagina; but there was evidence that the woman had a uterine fibroid, not a polypus. I wrote to my friend, who had sent her to me, and found what furnished an explanation of the difficulty. It was that he had examined her during the loss of blood; and it was only necessary for me to wait a few days till it recommenced; and then there was a great fibroid, partially enucleated, down in the vagina, with tremendous flooding. That was not an intra-uterine tumour, according to the principle I have adopted of naming tumours according to their origin, but it was an embedded tumour in the course of spontaneous cure by enucleation.

An important point I must now mention: it is that, for reasons which I cannot divine, you have two distinct sets of cases —one, in which the cavity of the uterus is open and expanded;

and another, in which no enlargement has taken place beyond what is required to contain the tumour. You will understand that the former are much more easily dealt with as to diagnosis and treatment than are the latter, where you have to force your way every step you make. In the former class of cases you have only to open the neck of the womb, and you can feel all the uterine cavity; while in the other class you have to force your way every fraction of an inch you progress in making the diagnosis.

All the tumours I have been discussing in this lecture are diagnosed and treated very much in the same way. I have said that an intra-cervical tumour is generally spoken of as intra-uterine; it is easily diagnosed and treated; but it is quite a different matter when we come to intra-uterine tumours proper, and we have had several examples in "Martha" of the difficulties attending their diagnosis and treatment.

Suspicion, which does not reach the length of diagnosis, arises when you find an enlarged uterus, especially if it be also a little deformed; but if the uterus be much deformed, it is probable that the tumour is not intra-uterine. If the tumour is small you may have no evidence of enlargement even. Suspicion is first aroused in most cases by the occurrence of loss of blood, which may take place at the monthly periods, or altogether apart from them. This loss of blood it is which, in most cases, impels you to examine, per vaginam, in order to treat satisfactorily; for, without a complete diagnosis, treatment is very unsatisfactory. In some cases the intra-uterine tumour produces, in addition to loss of blood, copious serous discharge, or sometimes purulent discharge. I have seen several cases of intra-uterine fibroid in women, after the menopause, where the discharge was not bloody, but evidently from an inflamed uterine cavity, and so profuse as to have effects upon the constitution very nearly as powerful as loss of blood.

How are you to make sure of the presence of an intra-uterine tumour? First, you are not to attempt to make sure unless you have sufficient reason; for the process of making sure is itself attended with considerable danger—the danger of septicæmia from the injuries the process may cause; the danger of parametritis or perimetritis; which must always enter into our consideration. Let us suppose, however, that the case is serious enough to demand that you procced. You must get your finger into the inside of the woman's uterus to feel it. Examination by the probe is often spoken of, but it is utterly unsatisfactory: there is only one sort of probing that is conclusive for this kind of diagnosis, and that is with the living, educated

finger; the other hand aiding by acting in the bimanual method. This is especially successful in cases where the cavity of the body of the uterus is dilated: then, you may be able to insert your finger without further ado, without previous artificial dilatation; or you may, by the exercise of a little force, push the finger through the external and internal orifices; or, again, you may succeed by pressure, while the neck of the womb is held in a vulsella, to prevent its receding before your finger, or to pull it down on your finger. Some of you may remember that in "Martha" very lately we examined and diagnosed a case in this way. But generally, and invariably in that class of cases where there is no dilatation of the cavity, you have to dilate every particle of the neck and cavity which you wish to explore. Now, dilatation for the purpose of exploration of the cavity of the body of the uterus, when the cavity is not previously enlarged, is a much more difficult matter than is generally supposed, often only imperfectly done. You can push in your dilating apparatus, and keep it in by plugging the vagina, which will dilate only as far as the tent goes; and it is a very natural matter to be deceived, and think you have reached the fundus uteri when you have accomplished no such thing. I have often known this happen; it did so in "Martha" not long ago. We thought we had sufficiently (not completely) examined a woman's uterus; and it was only under the influence and urgency of her extreme danger from bleeding that we did so again, more thoroughly, and we found a little intra-uterine tumour, which we removed, with the best results.

The best method of dilatation is by means of tangle tents. Sponge tents are often used; I prefer the tangle, meantime at least. You must have a tangle tent at least three inches long; because, as actually happened in a case which I mentioned in a former lecture, where there was a little intra-uterine mucous polypus in an expanded uterus, the tangle tent may slip right into the uterus and become lost there, owing to its being too short. A uterus which is much hypertrophied may require even a longer tent than one of three inches to open it thoroughly.

Let us suppose that you have completed the dilatation; you have next to introduce your finger into the cavity so as to touch the fundus, and for this purpose you will probably require to hold or pull down the cervix with a vulsella, upon your finger, in the same way as you pull a glove on a finger. In one case we could not, in this way, arrive at a diagnosis, because the finger was not long enough to reach a mucous polypus, which was discovered only after the death of the pa-

tient, that took place from another disease altogether. In that case the cervix uteri was pulled down upon the finger as far as was possible, and yet the polypus was not reached. The uterus from os tincæ to fundus was four inches and a half long. Had it been a matter of extreme urgency to complete the diagnosis, the only way open to us would have been to push the fundus uteri down upon the finger from above, as in bimanual examination. In this manner I might have managed a case in which it was of importance to complete the examination. This method was not successful in the case just referred to. It was tried.

It is quite easy to understand this difficulty when you are looking for a small mucous polypus high up in a womb which is occupied by a fibrous tumour. Polypi of this kind, growing in spaces left uncompressed by a fibrous tumour, are not rare, and I have seen great good result from their removal; but in many such cases it is mechanically impossible by any known means to reach the polypus so as to diagnose or treat it.

I now come to speak of the treatment, and I recommend you to trust in "avulsion." Do not first separate the tumour and then take it off, but use avulsion, doing the two parts of the operation simultaneously. In the great majority of cases nothing else is required. You seize the little tumour, with a vulsella, and with a slight amount of rotation pull it out: it is, if a fibroid, enucleated by the violence. Of course, if it is a fibroid and already partially enucleated, it comes away with no difficulty; but even if it is covered by a thin capsule, by seizing it you can get it away without much trouble. If you should require any cutting, I recommend you to use a pair of curved scissors, though this is very seldom necessary in the case of a fibroid. In the case of a soft mucous tumour which is not a polypus the process of removal resolves itself, involuntarily on your part, into one of torsion and pulling away. You seize the tumour with a pair of uterine dressing forceps, and pull it off just as you would pull off a nasal polypus. In both sets of cases the process is essentially one of avulsion. In the case of adherent placental masses you peel off with your nail or with the tip of your finger.

I have treated a large number of these cases, but have never resorted to any means other than those above mentioned. Were I to do so, I should throw a wire round the neck of the tumour and burn it off with a galvano-caustic apparatus. I have no particular objection to the écraseur, but I think the other is the nicer operation, and by it you have security against bleeding if you do not cut off the growth too quickly.—*Medical Times and Gazette, Feb.* 7, 1880, *p.* 143.

90.—ON INTRA-UTERINE MEDICATION.

By Dr. W. S. PLAYFAIR, F.R.C.P., Professor of Obstetric Medicine at King's College.

Various procedures have been employed for the purpose of applying caustic or alterative medicaments to the interior of the uterus, either in the form of ointments, or of solid substances which melt in the uterine cavity, and spread themselves over its lining membrane. Many instruments have been invented for this purpose, by some of which a piece of solid nitrate of silver or some other substance is pushed into the uterine cavity and left there (a method of treatment strongly recommended by Courty of Montpelier); by others, a medicated plasma or ointment is similarly introduced. Sticks or crayons of various substances, such as sulphate of zinc, alum, and the like, are by some inserted into the cervical canal, and allowed to melt. I have made many experiments with these plans, and have long discarded them, believing them to be far more troublesome to use, more painful to the patient, and less efficacious than the method of applying the chosen medicament to the uterine cavity in a fluid form, and swabbing its mucous membrane freely with it, which I shall now briefly describe.

For this purpose, I use small probes of flexible metal, roughened with emery paper, two inches and a half in length, and attached to a wooden handle. Before operating, three or four of these are covered with a thin film of cotton-wool. A small piece of cotton is flattened between the palms of the hands; the probe is dipped in water to make the cotton adhere, and, by a little practice, the cotton is wrapped round it so firmly that it cannot be stripped off by any ordinary force, and so smoothly and evenly as scarcely to add to the bulk of the probe. The patient is placed on her left side, and the cervix exposed by a full-sized Ferguson's glass-speculum, which answers better, I think, than either the duck-billed or glass-speculum, as it embraces and steadies the cervix. Before commencing, it is advisable to pass the sound at least once, to ascertain the direction of the uterine cavity, and the probes must be bent to accommodate themselves to it. With one or two of the mounted probes, the cervical and uterine cavities are thoroughly swabbed out, so as to clear them as much as possible of the discharges which might otherwise prevent the medicaments used from reaching the mucous membrane efficiently. If the discharge be very tenacious and adherent, it may be advisable to moisten the cotton with water or glycerine to facilitate its removal. Another probe is then dipped into the solution to be used, and the cavity is thoroughly swabbed with it. In the majority of cases of endometritis, there is no diffi-

culty whatever in passing the probes, since the cervical canal is abnormally patulous, and they enter with the greatest ease. Indeed, increasing contraction of the cervical canal, and consequent difficulty in passing the probes, is frequently a sign of improvement. Dr. Atthill, in the cases in which he applies fuming nitric acid to the uterine cavity, presently to be considered, uses a small vulcanite speculum, which he passes into the canal of the cervix, and through which the probe is introduced. I believe, however, that the use of this unnecessarily complicates the procedure, and that it has no great practical advantage.

Such is the plan of intra-uterine medication I myself adopt; and I now proceed to say a few words as to the agents used and the time for using them. Before doing so, I would wish to make a preliminary observation of great importance, which is that, even in cases otherwise eminently adapted for intra-uterine medication, it is often a matter of moment to prepare the uterus for it by certain preliminary treatment. This is of peculiar importance in cases in which there is appreciable tenderness of the organ on examination, indicating some degree of active congestion. A few days, or even a week or two, devoted to relieving this will be well spent. No agent is so useful for this purpose as the nightly application of a tampon of cotton wool well soaked in glycerine, an invaluable addition to our resources in the treatment of uterine disease, and one which has the advantage of being capable of being employed by the patient herself. Frequent hot-water douches, acupuncture, or the application of one or two leeches to the cervix, are also often useful preliminary measures.

A great variety of substances have been employed. Amongst these may be mentioned nitrate of silver in solution, tincture of iodine, chromic acid, carbolic acid, solutions of the sulphate of zinc or copper, the acid nitrate of mercury, and fuming nitric acid. After many experiments, I have arrived at the conclusion that the safest and most reliable agents are tincture of iodine, and a mixture of equal parts of crystalline carbolic acid and glycerine, the latter being that which I generally use. I conceive that what we require in most cases is not a solution which will cauterise and, if sufficiently strong, destroy the tissues to which it is applied, but one which will so modify the circulation as to cause a healthy reparative action in the diseased structures. Hence some of the substances I have enumerated above are too powerful for ordinary use, and, in fact, do more than is required. No better evidence of this is needed than the case recorded by Mr. Wigglesworth in the Obstetrical Journal for January 1879, in which the application of fuming nitric acid was followed by cicatricial contraction and complete occlu-

sion of the os, and subsequent very serious results. Now, carbolic acid has the advantage of being a powerful alterative, causing shrinking and mummifying of the tissues when applied in a concentrated form, but never producing an eschar. It is therefore free from all risk of producing cicatricial contraction. When I first commenced the practice of intra-uterine medication, I was in the habit of employing it once, or sometimes twice, a week, without reference to the menstrual period. I soon found that, when used too near the period, it was very apt to bring it on prematurely, and sometimes profusely. I therefore now limit its use to the first ten days after menstruation; and I find, as a rule, that two applications, at the interval of three or four days, are quite as effectual as a larger number, and much less irksome to the patient. It is probable that they have a better effect at this time, since the superficial layer of the endometrium, shed during menstruation, has not yet been reformed, so that the alterative action of the remedy more directly affects the deeper layer of the mucous membrane. In the more common cases, occurring after parturition, in which the cervical canal is patulous, such a course of treatment, systematically carried on after three or four successive periods, will generally prove effectual in relieving the patient from her sufferings. In the rarer cases in a nulliparous patient, with a conical cervix and contracted os, much greater difficulty is experienced, and the result is frequently disappointing. The amelioration is often only temporary, and, after a time, the symptoms are apt to recur as severely as ever. It has been pointed out, especially by Thomas and Lombe Atthill, that the reason of this is, to quote Dr. Atthill, "that the conical cervix and contracted canal, coupled with anteflexion, prevent the menstrual discharge and viscid secretions of the diseased lining membrane from obtaining easy exit. The first step, then, towards effecting a cure is to ensure the free escape of the contents of the uterus. There is but one way of effecting this—the cervix must be divided." This, of course, renders the case much more complex; and, as I am always unwilling to be over-active in treatment, it was long before I could bring myself to believe that this procedure might be necessary. I have, however, seen such beneficial results follow in cases in which the more ordinary method had been long and carefully tried, that I am constrained to admit that division of the cervix is, in some cases, absolutely indicated. The full consideration of this subject is, however, of too much importance to admit of being treated in a cursory manner, and I merely allude to it in passing.

I have hitherto said nothing of general treatment; not because I underrate it, but because my special object, at present, is to consider the local management of such cases. Suitable

constitutional measures, varying according to the exigencies of each individual case, are of the greatest moment, and will always materially facilitate the cure.

So, again, any complications, more especially the flexions which are frequently met with, should receive careful attention. It is totally impossible for me to endorse the theory of my friend Dr. Graily Hewitt, that endometritis is simply a secondary result of uterine flexion; but I do not differ essentially from him in my estimate of the beneficial results to be obtained from the judicious use of mechanical appliances when the uterus is distorted, which can be used *pari passu* with intra-uterine medication. What I maintain is, that those who rely too exclusively on one or other deprive their patients of an important aid to recovery, and that the truly scientific method is to get the maximum of benefit from both combined.

I have alluded very briefly to the employment of fuming nitric acid. The use of this agent originated in this country; it has been strongly advocated by the Dublin school of gynecologists; it has been as strongly protested against by others, who have probably given the matter but little thought, and who have no practical knowledge of its effects; and, therefore, I feel that I ought to state my own opinion with regard to it. The suggestion that fuming nitric acid should be freely applied to the interior of the uterus, at first almost horrified by its boldness, and the most disastrous consequences were predicted. The fact that such men as Kidd, Atthill, and many others testified to its safety and utility, of itself was sufficient, to my mind at least, to ensure for the proposal the most respectful consideration, and I have not been backward in giving it a fair trial. My conclusions as to its use I will briefly state. In endometritis with profuse catarrhal discharge, I believe that nitric acid is in no way superior to milder and safer remedies; and, although I have used it in a sufficient number of cases to justify my forming an opinion on the point, I have not satisfied myself that it possesses any advantages which counterbalance the bad effect which may possibly follow the application of so powerful an escharotic. It seems to me to be open to the objection I have already indicated, that it is apt to do too much; that it tends to destroy the tissues to which it is applied, while, in fact, our aim ought to be to modify vital action, not to produce an eschar. In cases of severe and otherwise intractable menorrhagia, in which nitric acid has been chiefly used, it stands on a different footing. In them, it is assumed, and I believe with considerable probability, that the entire lining membrane of the uterus is in a thickened, rough, and granular condition; and here the destruction of its lining membrane may have a distinctly bene-

ficial effect. The evidence produced on this point by our Dublin colleagues seems to me too strong to resist; and, indeed, my own experience, so far as it goes, is distinctly favourable. I have seen many cases of otherwise intractable menorrhagia completely and rapidly cured by a single application of nitric acid to the uterine cavity. It must not be forgotten, however, that there is a distinct risk of producing occlusion of the cervical canal. It is true that in no case coming under my own observation have I seen the slightest ill effects following the use of this agent. Still, the one I have cited from Mr. Wigglesworth is, of itself, sufficient to prove that the risk is not entirely imaginary. I am not sure that preliminary dilatation of the cervix is essential. I have often used it without this being done, and with the best results. I have never, however, applied nitric acid unless I had satisfied myself that the cervical canal was abnormally patulous, so that some degree of contraction would be of little consequence; and, unless this were the case, I should prefer to dispense with this plan of treatment. I am by no means sure, moreover, that the application of nitric acid possesses any advantages in cases of the kind over the use of the dull wire curette, as recommended by Thomas, Mundé, and others; a simple means of removing any granulations of the mucous membrane which is painless, requires no preliminary dilatation of the cervix, and is oftentimes very effectual.

Before concluding, it is necessary to consider whether intra-uterine medication is attended by any risks which might outweigh the good effects to be derived from it. If roughly and injudiciously used, I can fancy that intra-uterine, like any other form of uterine treatment so carried out, might produce serious inflammatory mischief. When judiciously applied in suitable cases, I am satisfied that no risk of the kind is run. I have been daily practising intra-uterine medication for many years, and I have only in one case seen any trouble of the kind. Every gynecologist is, however, aware that even the simple passage of the uterine sound may, now and again, produce a like effect; and certainly the possibility of this is no argument against the use of that instrument. Practically, the only bad effect that is at all likely to occur is the occasional production of a hemorrhagic discharge. This most commonly happens either when the treatment is first commenced, presumably because the mucous lining of the uterus is then most likely to resent any mechanical interference, or when the application is made too near the expected advent of menstruation. The discharge, however, never lasts beyond a day or two, and never leads to any serious trouble.

Mr. Wigglesworth has stated his fear that intra-uterine medication might produce sterility, by destroying the uterine mucous

membrane. If there were any foundation for this (and Mr. Wigglesworth brings forward no cases in support of his theory), it would, of course, be a very strong argument against the practice. As a matter of fact, however, I am satisfied that the apprehension is quite groundless. So far from this being the case, I have long been in the habit of considering the cure of chronic endometritis by intra-uterine medication as one of the most certain means of removing acquired sterility. I have seen so many cases of the kind that, as I stated in a short paper, published in the Obstetrical Journal of February last: "It has been a matter of every-day experience with me to meet with many cases of chronic endometritis, with subinvolution, after a labour, perhaps years before, in which intra-uterine medication has been followed so rapidly by impregnation as to leave no doubt of the result being due to the removal of the cause which led to the sterility. So commonly have I found pregnancy follow the cure of these conditions, that I have over and over again remarked to patients, who have expressed themselves as rather aggrieved at finding themselves in the family way, that I had come to look upon pregnancy as the nearly certain result and proof of a satisfactory cure." It may suffice to dispose of this theory if I state that, since writing that paper, only a few months ago, I have had no fewer than five cases in my own private practice in which pregnancy has rapidly followed the use of intra-uterine medication in patients who had been long sterile, in one case eleven years, in another seven, having elapsed since the last pregnancy.—*British Medical Journal, March* 27, 1880, *p.* 468.

91.—INTRA-UTERINE MEDICATION.

By Dr. EDWARD JOHN TILT.

Intra-uterine medication should be reserved for very exceptional cases of the following forms of disease.

1. Incoercible blood-loss, resisting milder remedies and menacing life;

2. When internal metritis menaces life or reason, rather by the intensity of its reaction on the system, than by the amount of purulent discharge; for insanity may be sometimes fairly attributed to an impairment of brain-tissue, due to long protracted internal metritis;

3. When, independently of ovaritis, internal metritis leads to an aggravated combination of dysmenorrhea and menorrhagia menacing life or reason;

4. Membranous dysmenorrhœa of exceptional severity;

5. Habitual abortion, independent of syphilis and ovaritis, and seemingly caused by some morbid condition of the lining membrane of the body of the womb.

It is now admitted, as an essential preliminary of all intra-uterine medication, that it is indispensable to establish a free passage through the cervix for all fluids injected into the womb or by it secreted; and I have found the dilatation of the whole cervical canal by laminaria-bougies, a day or two before menstruation, to diminish greatly the pain and blood-loss attendant on membranous dysmenorrhœa and the uterine irritation that follow the expulsion of the membrane.

With regard to treatment, when internal metritis is leading to death by blood-loss, I recommend the injection of the undiluted tincture of iodine; for, by doing so, I have repeatedly checked flooding, and also uterine exfoliation in membranous dysmenorrhœa. Tincture of iodine commends itself by antiseptic as well as by hæmostatic properties, and by the fact of its having been found trustworthy, for the purpose under consideration, by Drs. Savage, Routh, Fordyce Barker and Emmet. I have only heard of its use having been followed by one fatal issue, in Dr. Gaillard Thomas's hands; I am not aware of its having led to any pelvic disease of a serious nature; and a manageable amount of acute metritis is the worst result I have observed in my own practice. Similar immunity from serious results cannot be recorded of the injection of a solution of nitrate of silver in cases of internal metritis. The plan has often led to death, and still more frequently to pelvic peritonitis, from which patients have recovered with difficulty. I am struck by the fact, that the liability to serious disease after using intra-uterine injections is not proportional to the energy of the fluid injected, for formidable accidents have followed the injection of glycerine and of plain water. For that reason, when internal metritis has necessitated intra-uterine medication, I have sometimes had recourse to the solid nitrate of silver, and I have dropped into the womb about five or six grains of it, by means of Simpson's "porte-caustique"; thus returning to what Recamier did fifty years ago. I have seen this followed by acute peritonitis; and, as there are cases on record where death has ensued, I am ready to welcome a better plan of treatment.—*British Medical Journal, March* 27, 1880, *p.* 470.

92.—INTRA-UTERINE MEDICATION—IODISED PHENOL.

By Dr. ROBERT BATTEY, Fellow of the American Gynecological Society, &c.

Eight years ago, I was impressed with the opinion that the results obtained from intra-uterine medication by argentic nitrate and other escharotic remedies, as was then the custom in America, were very unsatisfactory. In my own practice, it

was a common observation, that scanty menstruation of a permanent and intractable character followed upon the treatment, due apparently to a cicatricial condition of the endometrium left behind. In not a few cases, stenosis of the os had to be remedied, and in some instances recurred time and again. In a few cases, entire occlusion of the os occurred, and retained menses had to be evacuated.

In casting about for eligible substitutes, the iodine tincture and carbolic acid presented themselves, and were successively tried, both separately and in combination, but the results thus obtained were meagre and unsatisfactory. Theoretically, iodine appeared to offer decided advantages, not only as a local stimulant to the uterus, but, in consequence of its ready absorption, as a local and general alterative also, but the officinal tincture proved too feeble in power to secure satisfactory results, and the stronger preparation of Dr. Churchill of Dublin was to me then unknown.

The thought of employing carbolic acid as a solvent for iodine suggested itself, and experiment developed a knowledge of the remarkable solubility of the latter in the liquefied acid. At first, one drachm, then two, three, and four drachms of iodine, was found to be soluble in an ounce of the acid. The last, and strongest, solution proved to be decidedly escharotic in its action upon the tissues, and especially upon heterologous growths of low vitality, and has been much used by the writer for attacking uterine cancer, and more particularly to supplement the curette. The standard solution employed in intra-uterine medication consists of one part by weight of iodine dissolved in four parts of liquefied carbolic acid, and to this solution I have given the name iodised phenol.

Iodised phenol is believed to be simply a concentrated solution of iodine in carbolic acid, and not in a proper sense a chemical compound. It is black in colour, syrupy in consistency, and possesses in marked degree the pungent odour of iodine, which is rapidly given off when it is heated.

Since its introduction into my practice, the iodised phenol for intra-uterine medication has been employed by me to the almost entire exclusion of other remedies. In Feb. 1877, it was brought to the notice of the profession in America through the columns of the American Practitioner, and is to-day very much employed, but more especially in the southern states.

At first, it was employed in a state of more or less dilution with glycerine; but, more recently, it has been used only in its full strength, being the energy of the application, regulated by the quantity employed and the extent to which it is carried into the uterine cavity.

The instrument employed in making the application may be

one of the many forms of applicators, so-called, or any uterine probe or sound which will easily enter the canal. It is my habit, and I specially prefer, to use a rather slender and elastic hard india-rubber probe, made slightly tapering, and with a blunt, not bulbous, point. The elasticity of this probe allows it to yield rapidly to pressure, to change its course, to follow easily the canal of the cervix, and to enter the uterine cavity proper, and this in spite even of a moderate flexion or version of the uterus. From the cotton-factory is obtained cotton wool in the form of an untwisted rope or roll, the fibres of the cotton being perfectly straight, and lying parallel to each other. It is technically known to the cotton-spinners as "the lap," and can be purchased of the best quality at our factories for eightpence to tenpence sterling per pound. It is admirably suited for general gynecological uses.

Mode of Application.—Having selected six or eight of the elastic probes, I break off from the cotton "lap" four or five inches, and with my fingers separate or split it into several fasciculi of such size as, when wound upon the probes, will enlarge them to the thickness desired. The end of a probe is now moistened slightly, and the fasciculus of cotton wound spirally upon it. The cotton-armed probe is now dipped into the iodised phenol, any redundancy is allowed to drip away, and the probe is passed into the uterus with a slow spiral movement as it advances. At first, the probe is introduced but a short distance, and immediately withdrawn, and the case rests here to test the tolerance of the uterus for the remedy. At subsequent stages, the probe may be carried to the fundus, and followed immediately by a second, and even by a third or fourth, if well borne. The remainder of the wrapped probes are employed for wiping off the cervix or vaginal wall any of the phenol that may have touched these tissues. The energy of the application is regulated by the size of the wrapping, the depth to which the probe is passed, and the number of medicated probes used. When a very decided impression is to be made, a backward turn is given to the probe in its withdrawal, so as to leave the saturated cotton in the uterus, there to remain twenty-four hours, or even until it is spontaneously expelled. The application is renewed every four to fourteen days, according to the energy of the treatment.

I have abandoned the use of sponge-tents in connection with the treatment set forth. When dilatation is required, the cotton-wrapped probe is employed, and the cotton left as a soft tent in the canal. The dilating power of this is notably less than of sponge, but nearly equal to sea-tangle, and, it is believed, entirely safe. The results are the following.

1. A perfect removal of all cervical mucus, which is promptly

coagulated, and comes away closely adhering to the cotton. The probes subsequently passed bring the remedy directly in contact with the diseased membrane.

2. Always comparative, and usually entire, freedom from pain. This is a marked feature of the method, and in striking contrast with former experience. Carbolic acid is a local anæsthetic, and so numbs sensibility as to make the energetic application of iodine for the most part entirely devoid of pain.

3. The iodine is so rapidly absorbed by the uterus, that the patient remarks its metallic taste in the mouth and throat, ordinarily in five or ten minutes after the application.

4. Softening and more or less dilatation of the cervix and os.

5. There is temporary arrest of leucorrhœa, followed by

6. Watery discharge, sometimes bloody.

7. There is exfoliation of the superficial layer of the mucous membrane, which comes away in shreds, sometimes entire, and resembles glove-kid.

8. Abrasions of the os promptly heal.

9. Indurations of the uterus disappear.

10. Leucorrhœa is permanently arrested.

11. Villosities of the endometrium are removed without resort to the curette.

12. Subinvolution of the uterus disappears.

13. The menses become regular and healthy; menorrhagia and scanty menstruation, as well as dysmenorrhœa, are remedied.

14. The appetite and digestion improve, and this, in many instances, without the use of medicines.

15. So thoroughly is the system impregnated with iodine, that alteratives by the stomach are not used.

16. The form of the cervix and os is often completely changed; a large puffy cervix, with patulous slit-like os, becomes even virginal in type after long use of the remedy.

17. Stenosis has not followed the treatment in any case noted.

18. Barrenness of nine to fourteen years' duration has been removed in several instances.

Remarks.—Rapid, and at the same time satisfactory, cure of chronic uterine ailments, such as are contemplated in this paper, is not attainable by any method of treatment known to me. It is not proposed that rapid cures can be made by the means herein set forth; on the contrary, the long standing and obstinate cases, such as usually fall into my hands, require many months for satisfactory cure.—*British Medical Journal, March* 27, 1880, *p.* 471.

93.—ON A NEW INSTRUMENT FOR INTRA-UTERINE MEDICATION.

By Dr. J. W. MILLER, Dundee.

Dr. Lombe Atthill, in his communication to the Journal of Dec. 13, on intra-uterine medication, makes an observation which induces me to send a short description of an instrument which, for some years, I have found very useful in overcoming the inconvenience to which he alludes, namely, that, in the introduction of the probe armed with cotton-wool soaked in carbolic acid or other fluid, much of that fluid is wiped off before the instrument reaches the part upon which it is desired to act. To avoid this, I at first used an ordinary flexible catheter with a piece cut off its extremity, but this was not well suited for repeated use.

I therefore requested Mr. Young (of Edinburgh) to make this instrument for me, which is a modification of the late Sir James Simpson's porte-caustique. It is a silver cannula, eleven inches long, of the calibre of a No. 6 catheter, bent at an obtuse angle three inches from its extremity, and having a knob on its convexity two inches and three-quarters from its extremity like that on an uterine sound. It has a stilette two inches and a half longer, the projecting portion slightly roughened, terminating in a round knob, which, when the stilette is drawn in, closes the end of the cannula, facilitating its introduction, and confining the fluid with which the cotton wool on the stilette is soaked. The stilette has also a sort of wedge-shaped collar, two inches and a half from its extremity, to enable it to be drawn back into the cannula before the instrument is withdrawn from the uterus. The wool should not be wrapped on the stilette too tightly; and in drawing the stilette with the soaked wool into the cannula, it should be guided in with the fingers, so as not to allow wool and fluid to be scraped off on the end of the cannula. The button-extremity of the stilette now completely closing the end of the cannula, the instrument can be passed up to the uterine cavity without the fluid coming into contact with any part of the passages. After reaching the fundus, the cannula is retracted on the stilette (not the latter pushed forward), and the fluid then comes into free contact with the mucous membrane. Another advantage of the instrument is that it may be employed without the aid of the speculum. It may be also used for the introduction of an ointment or powder, by drawing the button-like extremity of the stilette into the cannula so as to leave a space into which either of these may be packed. For convenience, the instrument might be named an "intra-uterine medicator."—*British Medical Journal, March* 27, 1880, *p.* 472.

94.—ON INTRA-UTERINE MEDICATION.

By Dr. CLEMENT GODSON, Assistant Physician-Accoucheur to St. Bartholomew's Hospital, &c.

There is much in fashion. A century ago, every one was bled; now venesection is hardly ever practised. We have passed from one extreme to the other. A few years since, in gynecological practice, division of the cervix by the metrotome was constantly performed by some physicians; now, this operation is but rarely undertaken; indeed, we hear, at least in one quarter, that sewing up the gaping os uteri is all the rage. Lately, there has been a discussion on the use of the forceps and its alternatives in lingering labour, before the Obstetrical Society of London. Many eloquent discourses have been contributed, and the subject has been ably discussed by several of our eminent leaders in the school of midwifery. But, to sum up the whole, at what have we arrived? The general impression gained, is that the forceps is used far less frequently than it might be with advantage to both patient and practitioner. The result of this will be, unless I am much mistaken, that an authority has been granted for the frequent use of the instrument, and that we shall find the evil effects of this from its indiscriminate and unskilful employment. In the out-patient female department of St. Bartholomew's Hospital, where I see from fifty to seventy new patients per week, I have unusual opportunities of observation. After having made an examination, I am frequently led to inquire: Were you delivered with instruments in your confinement? And, in nine cases out of ten, the answer is in the affirmative. The reason why I put the question was that I found the cervix deeply fissured up to its junction with the vaginal wall, along which extended a cicatrix. This is a clinical fact, which testifies to the truth of the remark made by Dr. Henry Bennet in his speech, and objected to by Dr. Barnes in his reply, at the Obstetrical Society's discussion.

But, with respect to uterine medication, the results are likely to be much more serious; and I have already met with instances. If an application to the cavity of the body of the womb be required, there should be an amount of *endometritis* calling for it; but, in how many cases of discharges from the os uteri does the exudation come from above the canal of the cervix—the so-called internal os? I venture to say, in very few. The probe, covered with cotton wool saturated with carbolic acid, is passed along the cervical canal up to the fundus uteri, and in many cases good results are obtained—not because the fundus uteri has been reached, but *because the application has been made to the canal of the cervix;* and, had a zinc-alum point

been placed in this situation, and the uterine cavity have not been reached, the result would have been equally satisfactory, without the risk of danger possible to occur from probing the cavity of the womb. This brings me to the nature of the evil consequences.

One which I have met with is abortion. The possibility of existing pregnancy has not been thought of. The patient may have gone but a few days over her time for menstruation, and, not having been very regular previously, little heed is paid to this. Nevertheless, she may have conceived immediately after her last period, and, if even she have not quite reached her next menstrual epoch, she may yet be pregnant when the application is made. Another danger is that of inflammatory mischief in and around the uterus. It is well known how the simple passage of the ordinary uterine sound is liable to give rise to this. Surely, then, a probe saturated with carbolic acid or other agent is equally if not more likely to produce such results.

The points, therefore, that I would urge upon my professional brethren are these. Before deciding to make applications to the cavity of the body of the womb, be satisfied that this part is affected by disease, and that it is not the cervical canal only that is involved. Without having determined this, it would be just as irrational to pass a medicated bougie into a man's bladder for every purulent discharge escaping from the meatus urinarius.—*British Medical Journal, Nov.* 8, 1879, *p.* 730.

95.—PATHOLOGY OF MAMMARY EPITHELIAL TUMOURS.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

The papers recently read before the Royal Medical and Chirurgical Society, on cancer of the breast and the peculiar diseased condition of the nipple, first pointed out by Sir James Paget as connected with that disease, have raised some interesting questions regarding cancers in general, and more especially regarding epithelial growths in the mamma. Dr. Thin has stated that, of the four tumours which he examined, all were examples of what he calls duct-cancer; and this duct-cancer he believes to be the same disease pathologically as that described as adenoma mammæ, or chronic mammary tumour. If this view be correct, there are facts that need explanation, for the elucidation of which more extended observations are evidently required. Cases of chronic mammary tumour or adenoma, notoriously, in a great number of instances, last a long time without producing much local or general disturbance, a course which contrasts with the history of the cases which supplied materials for Dr. Thin's investigations. The cancer of the breast associated with Paget's disease

of the nipple is without doubt a cancer that formidably threatens life.

It is now many years since Mr. Birkett called attention to the existence in the mamma of epithelial growths, which are distinguishable from scirrhous cancer of the organ, and pointed out clearly their salient features. So far back as in the second volume of the Transactions of the Pathological Society, he has described a case which recurred seven years after operation, the growth being found to consist of "acini or terminal vesicles of gland-tissue, very large, and distended with epithelium." The appearance thus described by Mr. Birkett is very frequently seen in these tumours, and is perhaps their most striking characteristic. The attention of observers has, however, perhaps been too exclusively fixed on this resemblance to gland-elements in the newly formed epithelium, changes in the connective tissue intimately connected with them being less accurately appréciated.

The concurrent testimony of the most experienced modern observers goes to confirm the doctrine that there are two distinct kinds of epithelial growth in the mamma. In one form, frequently recognised as scirrhus, the growth tends to diffuse itself rapidly through the connective tissue of the breast, and absorption by the lymphatics is the rule. In the other form, the epithelial growth is found in circular groups or clusters, surrounded by connective tissue, into which it has usually a comparatively slight tendency to penetrate; and around the circular cell-groups a distinct limiting membrana propria is often found—a fact to which German observers are prone to attach much importance. Dr. Thin, however, states that the membrana propria is not invariably present, and regards it as a new formation, frequently but not invariably found around the cell-masses. In scirrhous cancer the membrana propria seems never to be found.

The division of epithelial growths of the mamma into two classes, each distinguished by a special mode of development, governed by certain fundamental laws, and the tracing of each class to a distinct epithelium as the starting-point of the disease, are extensions of the doctrine first demonstrated by Thiersch for epithelioma of the lip, that cancerous epithelium is always a product of a pre-existing epithelium. The cancers of the mamma seem to afford an instructive illustration of that law. In the breast, there are two kinds of epithelium—the columnar epithelium of the ducts, and the secreting epithelium of the acini. One of the points insisted on by Dr. Thin is that the two varieties of cancer derive their distinctive qualities from these two kinds of epithelium.

The peculiarity of duct-cancer, it is stated, is the formation of rounded columns of cells, which sometimes form perfect

epithelial tubes—a process that in its rough outline seems to resemble a futile attempt at a new formation of gland-tissue. But, in this instance, the attempted formation is abortive; instead of a glandular we have a columnar epithelium; and rounded masses of cells, which frequently break down in the centre and produce certain characteristic effects on the surrounding connective tissue, are the only result.

A point of clinical interest is that tumours which, under the name of adenoma or chronic mammary tumour, have been regarded as non-malignant, do, in a certain number of instances, run a comparatively rapid course, and produce death. Waldeyer applied to these growths the term "carcinoma," and recognised their connection with the galactophorous ducts. Many cases of rapidly growing tumours have been probably confounded with scirrhous cancer. The much-quoted case reported by Pitha and Billroth supports this suggestion. The tumour was diagnosed and operated on as a case of common carcinoma, and it was only after careful histological examination that its true nature was ascertained, and that it was pronounced to be an "adenoma." Labbé and Coyne relate the case of a woman aged 67, in whose breast a painless tumour, of the size of an almond, remained stationary for thirty-four years. It then began to grow, and, after two years' continued active growth, it was removed. It had all the essential characters of a duct-cancer, and not those of scirrhus. The same authors report another case, in which a woman had a tumour that developed immediately under the nipple when she was between thirty and forty years of age. It remained small for thirteen years; then it began to grow, and in two years the skin ulcerated. Removal was followed by reappearance of the disease; this time accompanied by glandular affection of the axilla. There was no ulceration for two years; and, at the end of three years, the woman died. Histologically, the tumour was identical with the growths variously designated adenoma, chronic mammary tumour, or duct-cancer. The tumours connected with Paget's disease of the nipple were described, in the paper read before the Royal Medical and Chirurgical Society, as identical in structure with the so-called adenoma; and additional evidence thus appears to be forthcoming, to show that this usually comparatively innocent growth may sometimes run a malignant course. We infer that it also further illustrates the impossibility, so far as our knowledge at present goes, of always determining the nature of a tumour without microscopical examination.

Whether these tumours should retain the designations by which they have been hitherto known in England, is a question that is open to discussion. The term *adenoma* is apt to mislead, inasmuch as the new growth forms no secreting structure, and

does not even originate in the pre-existing secreting elements of the gland. The term *chronic mammary tumour*, or *chronic mammary glandular tumour*, perhaps, fails to convey an idea of the relative importance of the epithelial elements of the growth, and their occasional destructive effects. On the other hand, many surgeons will consider it inappropriate to apply the term *duct-cancer* to a growth that usually remains localised for many years without constitutional infection. But the progress of pathological knowledge has taught us the necessity of taking into account the anatomical structure of all tumours in questions of nomenclature. It is now universally admitted that every cancer consists of a growth of epithelium in the wrong place. But not every abnormal growth of epithelium is of necessity a cancer. To constitute a carcinomatous growth, the epithelial elements must exercise a destructive influence on the healthy connective tissue—an abnormal growth of connective tissue (the so-called stroma) usually taking place. This destructive effect of the morbid epithelium is not exactly the same in degree in any two cases of cancerous epithelial growth, even when the variety of epithelium affected is the same in both. Still more marked is this difference when different kinds of epithelium are involved in the morbid action. But the kind of diseased influence is the same. In one variety, the destruction of vascular tissue may be rapid, and the protecting effect of a barrier of newly formed connective elements almost *nil;* in another, the first destruction of the connective tissue immediately adjacent to the epithelium may, so far as the safety of the patient is concerned, be compensated by a strong barrier of membrana propria and fibrous tissue. The latter result is what frequently takes place in the formation of so-called adenomata, although not always to the same degree.

Judged by these tests, the chronic mammary tumour is a cancer. The growing epithelial tubes and columns not only displace, but destroy, the surrounding bundles of connective tissue (a process that can be well seen by an examination of specimens treated with osmic acid), and, as the columns fuse into the large spherical masses characteristic of the developed disease, the connective tissue between them is completely destroyed. Still, in most cases, the destructive effect on the vascular tissues cannot be compared to that exercised by the cancers that take their origin from the secreting epithelium. Frequently the epithelial masses are surrounded by a barrier of newly formed connective tissue, and, if Dr. Thin be correct, not unfrequently by the creation of a newly formed membrana propria.

The effect on the connective tissue varies very much in different instances. In many cases, it is comparatively slight and

leads to a capsular formation; sometimes, the newly formed tissue is incompletely developed as regards quality, but largely developed (compared with the epithelium) as regards quantity, and we have then the tumour which is described by Billroth as adeno-sarcoma. In some specimens, all these changes are found to constitute different stages of development in the same tumour. In other and rarer tumours, the destructive effects on the connective tissue are, in some parts of the growth, greatly in excess of any new formation of that element; and there is then a tumour that clinically is apt to be mistaken by the surgeon for scirrhus, and which differs as much from the small chronic mammary tumour as a small superficial rodent cancer does from the severe forms of that disease.

Assuming that the tumours of the breast associated with Paget's disease of the nipple are anatomically of this variety, the cause of the apparently large ratio of malignant types amongst those as yet reported forms an interesting subject of speculation; but at present it is one regarding which we can only speculate. Whether a growth, that springs from a morbid action connected with the more highly differentiated columnar epithelium of the large ducts, is more malignant than one that springs from the epithelium of the smaller ducts; or, whether mere proximity to the surface and consequent irritation of the diseased epithelium, in its earlier stages, endows the unknown cancerous agent with greater virulence, are questions that can as yet only be answered by hypothesis. —*British Medical Journal, March* 20, 1880, *p.* 446.

96.—NITRITE OF AMYL IN UTERINE HEMORRHAGE.

By ELIAS W. KERR, Esq., M.B., Bundoran, Ireland.

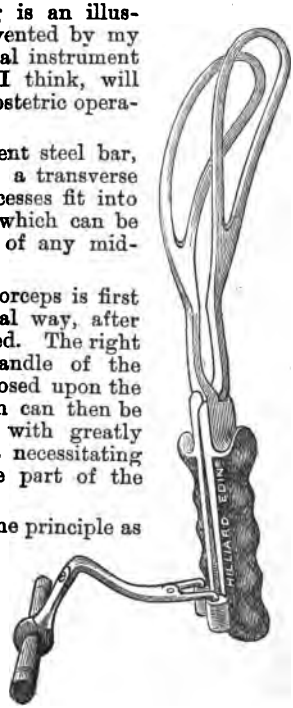
Dr. Koehler states that he has for the last seven years, in cases of uterine hemorrhage, applied warm fomentations to the head, to prevent anæmia of the brain and also of the heart. By this means, Dr. Koehler states that he has saved patients in the most dangerous cases of hemorrhage. Of the success of this method of treatment I can myself speak with confidence, and the *rationale* of it I take to be the rapid dilatation of the cerebral vessels; and, if so, have we not in nitrite of amyl a more rapidly acting and more powerful agent to produce a similar result? This idea occurred to me a few nights ago, when attending a case of rather severe *post partum* hemorrhage; and, happening to have some of the drug with me, I at once put my theory in practice by administering five minims by means of a Skinner's inhaler, and, I am happy to say, with

immediate and most satisfactory results. The hemorrhage ceased at once and permanently, and the patient was restored from a state of collapse. Indeed, in this latter respect, its effect was only comparable to that of the hypodermic injection of ether. I have not been able to find any notice of nitrite of amyl having been heretofore used with the above views; and, of course, a single case is hardly sufficient datum upon which to claim much; but I hope to be able to give the medicine a more extended trial, and shall be glad to publish the results.—*British Medical Journal, Nov.* 1, 1879, *p.* 691.

97.—OBSTETRIC FORCEPS.—A NEW APPLIANCE.

By Dr. R. HARVEY HILLIARD, London.

The accompanying engraving is an illustration of a new appliance invented by my brother, Mr. F. Hilliard, surgical instrument maker, of Edinburgh, which, I think, will prove of considerable utility in obstetric operations.



It consists of a peculiarly bent steel bar, with two hinged processes and a transverse wooden handle; these two processes fit into corresponding slots or sockets, which can be easily attached to the handles of any midwifery forceps.

In using the instrument, the forceps is first applied and locked in the usual way, after which the lever is readily adapted. The right hand grasps the transverse handle of the lever, while the left is firmly closed upon the handles of the forceps; traction can then be made in the proper direction with greatly increased leverage, and without necessitating any change of posture on the part of the operator.

The instrument is on the same principle as Tarnier's forceps, of which it is a modification. I have used it with great satisfaction, and believe it to be a valuable addition to the ordinary forceps.—*British Medical Journal, Feb.* 21, 1880, *p.* 287.

98.—A MODIFICATION OF SIMS' DUCKBILL SPECULUM.

By Dr. ALEXANDER DUKE, Assistant-Physician, Rotunda Hospital, Dublin.

I wish to bring before the notice of the profession a modification of the usual form of Sims' duckbill speculum, as sold by the instrument makers.

The blade is much flatter and somewhat broader, and the part connecting the blade with the handle has a bevelled ring to admit the finger, which takes off some of the strain from the wrist and gives greater facility for manipulation.

The advantage of the blade not being so deeply hollowed is that, when used while plugging the vagina, there is much less liability to catch in the plug and pull it out altogether, as frequently happens with the ordinary form. Anyone who has been obliged to hold one of the ordinary duckbill speculæ or one with a straight handle ("Simon's") during a sustained operation sometimes lasting more than an hour, as in vesico-vaginal fistulæ, &c., will, I am sure, appreciate the cross handle, which is movable, being only fixed by a screw, can be carried separately, and when required adapted to any sized blade, and which, by bringing the "flexors" into play, gives immense power of traction, and is not nearly so fatiguing as when the straight-handled modification or older-shaped duckbill is used.

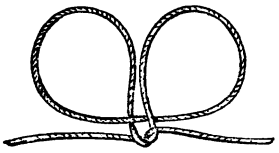
Messrs. Arnold and Co., Smithfield, are the London makers, and John Whyte, Dublin.—*Lancet, Nov.* 29, 1879, *p.* 802.

99.—OVARIOTOMY.—"TAIT'S KNOT."

By LAWSON TAIT, Esq., F.R.C.S., Birmingham.

The knot which Dr. Meredith has kindly named as above should rather be called the "Staffordshire Knot," as it is the badge of the county of Stafford; and the idea of its use occurred to me whilst travelling in a Staffordshire railway-carriage in which it was a conspicuous ornament. Mr. Mazzinghi, the learned custodian of the William Salt Library at Stafford, tells me that nothing is known definitely as to the origin of the badge, further than that it was granted by the College of Heralds to the county within living memory, and was copied from the arms of the old Stafford family at Maxstoke Castle. Such badges were used to distinguish the adherents of noble families, and their origin is generally altogether unknown. The legend in the present case is that rogues were at one time so numerous in Staffordshire that a knot had to be devised which would hang three at a time. If so, the original knot must have differed from its present representative, for the latter could only hang two. It is, however, its remarkable property in this direction which has led me to introduce its use in surgery.

The woodcut will show how the knot acts, and a very brief description will probably make clear enough how it is used. An ordinary-handled needle, armed with a long piece of the silk required, is passed through the pedicle and then withdrawn, so as to leave a loop on the distal side. This loop is then drawn over the ovary or tumour, and one of the free ends drawn through it, so that one end is above, while the other is under, the retracted loop. Both ends being seized in the hand, they are drawn through the pedicle, against which the thumb and forefinger of the left hand are pressed as a fulcum, till complete constriction is made. A simple hitch is then made, as in the drawing, and tightened; and that is followed by another, as in ordinary ligature-tying. There is another and more complicated way of making the knot, by passing each end of the thread round the corresponding half of the pedicle, and crossing them within the loop in front, which is equally effective, and which may be used in cases of large solid tumours. But the former way is by far the more elegant and rapid method.



The advantages of this knot over all others are that, while it ties the pedicle in two halves, these halves are compressed really into one surface; the two halves are equally well compressed; and, from the mechanical arrangement of the knot, very great constricting force can be employed; and, in this respect, it greatly excels Dr. Peaslee's chain-ligature. Finally, its application is very rapid, and everything which saves time in ovariotomy is very important. I have used it now a considerable number of times, and am satisfied it is a substantial improvement on anything else I have seen or used.—*British Medical Journal*, *Nov.* 22, 1879, *p.* 813.

100.—ON THE TREATMENT OF POST-PARTUM HEMORRHAGE BY HOT-WATER INJECTIONS.

By Dr. ATTHILL, Dublin.

The use of hot water in the treatment of this complication was very frequently employed both in the hospital and extern maternity, and has proved eminently satisfactory. It has, indeed, much to recommend it, for not only is it a powerful hæmostatic and excitant of uterine contraction, but it is also a general stimulant. If used with ordinary care it is not only harmless, but beneficial, by thoroughly cleansing the uterus from clots, portions of membrane, &c., which may have been left in its cavity. It will not, in Dr. Atthill's opinion, be

found altogether to displace the use either of cold water or of the perchloride of iron, but rather to be applicable to a distinct class of cases in which the former of those remedies would be unsuitable, and the latter unnecessary.

The method of carrying out the practice is exceedingly simple. An ordinary syphon syringe is the only instrument required, though we now use one with a long vulcanite nozzle, specially constructed for vaginal and intra-uterine injection. This is carried up to the fundus, and with the usual precautions against injecting air and securing a free return, we inject water as hot as can be conveniently borne by the hand, *i.e.*, about 112° F., in a full stream into the cavity, continuing thus until a good contraction is secured, and the water returns quite clear and colourless. The following are some of the results of our experience in the use of hot water:—

1st. In cases of sudden and violent hemorrhage in a strong and plethoric woman, it is better first to use cold.

2nd. Where, from the prolonged or injudicious use of cold, the patient is found shivering and depressed, the beneficial effect of injecting hot water is rapid and remarkable.

3rd. In nervous, depressed, and anæmic women, hot water may at once be injected without previously using cold.

4th. In cases of abortion where, from uterine inertia, the ovum, although separated from the uterine wall, is wholly or in part retained, the injection of hot water is generally followed by the most satisfactory results.

5th. Where the injection of the perchloride of iron is considered necessary, previous injection of hot water clears the uterus of clots, &c., permitting the fluid to come directly in contact with the bleeding surface, and lessening the chance of septic absorption.—*Annual Report of the Rotunda Hospital, Dublin Journal of Medical Science, Dec.* 1879.

ADDENDA.

101.—THE CANONS OF SANITARY WORK.

By Dr. ALFRED CARPENTER, C.S.S., Cambridge.

The basis upon which preventive medicine stands as a science is that of the *particulate nature of contagia*. It is the first canon law. We know that zymotic disease will not arise if the particle *z* is kept out of the process. Again, *z* cannot increase and multiply unless the material which has been used up in supporting the functions of the higher forms of life be also present. There are, therefore, two manifest duties for the sanitary authority to perform; first, to remove the pabulum *y*, upon which *z* is able to increase and multiply, from its neighbourhood to man; and also, so to alter its character that it is no longer able to nourish *z* if the particle should be imported; second, to prevent the importation of *z*, or if it be in our midst, so to alter its nature that it cannot fructify. These duties can be performed if the laws which regulate the introduction and the growth of *z* are well understood and counteracting agents regularly employed. There are two forms of germ, the evanescent and the persistent. Measures which may be successful against the former and may limit the spread of a present epidemic, will not necessarily prevent its recurrence in the future at the same place unless both kinds of germ are considered.

There are several ways by means of which the natural order *schizosporeæ* may make itself felt, viz., by the air, the water, the earth, or the food. The contamination of the air in the neighbourhood of our dwelling-houses both above and below the level of the ground must be counteracted. This contamination is effected in two ways: 1. By the ordinary processes of decay which are always going on in organic nature; and 2. By the aggregation of living creatures. Both give rise to excess of carbonic dioxide; without the latter, it is probable that fungi could not flourish, but it is animated nature which introduces myriads of the germs of debased protoplasm which is food for *z* to flourish in.

The higher the animal life is in the scale of creation, the more injurious is the excreta of such animals, and that of man most of all.

The remedy for these evils is *ventilation;* anything which

impedes it is contrary to sanitary law. *Motion* is the second most important fact, and is the *second law* of sanitary work. Currents of air must be established to lessen the quantity of carbonic dioxide to something less than four hundred and fifty parts in a million; and by introducing a fresh supply of oxygen, the albuminoid matters which living creatures give out may be altered in their chemical characters, and by that means they will no longer give renewed life to *z*. Motion of both air and water is a principal law of sanitary work. Air in motion is soon deprived of those organic matters which allow the growth of *z*, and the latter, as well as the food upon which it feeds, being very evanescent in its character, are soon changed into innocuous and even into useful material. This was very forcibly impressed upon my notice in the year 1854, when I ventilated the soil pipe of my own house by extending it upwards between the sewer and the trap; my successor in that house took away the soil pipe, and in a few weeks a death from typhoid fever occurred in that house. Stagnant air in the house-drains of this town produced more or less evil in almost every house in the place, until our local senators were convinced of the evil. It was in Croydon that the law for ventilating sewers was first put into operation by a local authority. But the local authority were then in advance of the intelligence of the people; and, although the command was issued, the work was not generally done until much more proof of its necessity was afforded. Now, the ventilation of the sewer and of the house-drain is required by the law of the land, but it is not nearly so general as it should be. Those openings into sewers, were at first called stink-pipes, and were sometimes taken away because they gave evidence that the law of motion was not complied with. The foul smell which came out, showed that the sewer was a sewer of deposit, that it contained stagnant sewage; and the neighbour, like the ostrich of the desert, who when danger threatens hides its head, takes away the safety valve, and tries to smother up the evidence which the stink-pipe gives out, and sends it into somebody's house, instead of insisting upon the removal of the foul sewage from the sewer. No smell will ever arise from a properly-constructed sewer, and if there be such smell, it is conclusive proof that there is deposit either in that sewer or in some other in close communication with it, or in the soil around it. No stagnation, either of air or of sewage, should be possible in any sewer.

Assuming that sewers are necessities in a thickly-peopled neighbourhood, they must not be allowed to ventilate into houses; and now a *third law* comes into operation, viz., that it shall not be possible for air to pass directly from a sewer into any house until it has been diluted by pure air and has had

time to have its albuminoid matters oxidised. *There must not be any communication directly between the sewer and the interior* of the house.

There is no occasion for any departure from this law, and it should never be allowed in practice.

There is another danger which arises from sewers if they are not constructed of impervious materials; brick sewers are open to great objection when they pass through pervious soil in close proximity to houses. Unless they are very freely and efficiently ventilated, they contaminate the air of the subsoil of a town until it becomes a perfect hotbed of mischief. This is a frequent cause for the continuance of enthetic disease in districts in which the water-line rises and falls at distinct intervals. The way in which gases travel long distances underground is sometimes shown by the distance which coal-gas travels when a fracture has taken place in a gas main. Sewer gases, which only form in badly-constructed sewers, are just as penetrating. The products of decomposition find their way through the bricks into the soil, and then into the foundations of the houses. There are some houses in this town which are built over sewers, and which must sooner or later be dangerous to the occupants. The builders of such houses ought to be compelled to notify the fact to every incoming tenant. Ventilation should be provided in every case in which a trap has been fixed, otherwise stagnation must arise. This is another fundamental law.

The next *law is that sewage must be utilised.* It must be conveyed as soon as it is formed away from the neighbourhood of human beings. *z* is found to flourish most luxuriantly in sewage which is about to undergo putrefaction, but in which putrefaction is not actually established.

The safety, as far as it is safe, of the old-fashioned cesspool out of doors arises from the fact that putrefaction is rapidly set up, and with it all danger from the multiplication of *z* is removed. This is also seen in the dissecting-room, or at *post mortem* examinations. There is much danger from dissection-wounds for a few days after a death; but if a student cuts his finger whilst dissecting a body in which putrefaction is advancing there may be an ugly sore, but there will not be a poisoned wound. In the cesspool, *z* may be indefinitely multiplied before putrefaction is established, and germs of mischief may find their way out of cesspools into wells and water-courses, and may do much mischief. No sanitary authority ought on any pretence to allow a cesspool to exist within its jurisdiction.

If there be no sewers, and I contend that they are not necessaries in a thinly-peopled district, excreta should be reserved in some utensil in contact with earth or some other deodoriser

able to arrest the changes which otherwise must take place, and before the power of change can reassert itself the material should be taken away and utilised in the ground. The pail system is far safer and far more economical for a village population than a system of sewers, and it is a serious blunder to introduce into small places the works which thickly-peopled cities require; but then the work of collection must be controlled by an efficient local authority. Sewage which has been removed from the town by water carriage must be applied at once to the land by means of surface irrigation. This subject has been repeatedly mooted at congresses, and by inquiries made by Royal Commissioners and Committees of both Houses of Parliament, and the general conclusion which has always been arrived at by unbiassed witnesses has been similar to that which was agreed to at the first sanitary congress ever held in this kingdom. In October 1866, the Leamington Sewage Congress, after a long discussion in a large assembly of scientific and practical men, agreed to the following resolution: "That the system of irrigation, when carried out in a scientific manner, removes the difficulty which now arises from the present noxious plan of polluting the rivers of England." A rider was added, which caused it to be unanimously accepted, viz.:— "But that there are circumstances in which other systems may be applicable." Thirteen years have passed away since that time, and I am still more convinced than ever of the truth of the following conclusions.

1. Sewers are necessities for crowded populations.

2. Having sewers, they are silent highways along which human excreta may be safely and efficiently removed by water carriage.

3. Sewer-gases and smells from decomposing sewage are not necessities of the system.

4. If sewage be judiciously applied to land, the spread of enthetic disease by its means becomes an impossibility.

5. After a proper application by surface irrigation, the effluent water may be safely discharged into the nearest watercourse.

6. The application of sewage to land must be conducted on scientific principles; otherwise failure, first financially, and then by producing nuisance, will arise.

7. A local board is the worst possible body to have the management of a sewage-farm. The management requires an intimate acquaintance with several sciences, with agriculture, with sale and barter, and an immediate personal command of capital: without these powers it cannot become a financial success. The manager of a sewage-farm must be an autocrat and not be liable to those attacks which are certain to be the lot of active members in a popularly elected local board. The

Croydon Local Board, with the best intentions, will have to reap the harvest which is sure to follow from the farm having too many masters; and the ratepayers of the parish who did not know when they were well off, and who are primarily responsible, will have to pay the ultimate cost.

The eighth conclusion at which I have arrived, is that a local authority, in providing for the utilisation of sewage by irrigation, must be prepared to pay the difference in value between the price of the land and its ordinary agricultural worth. One penny in the pound on each rate, or at the most threepence for the year, ought to be amply sufficient for this purpose, as well as for payment of interest on any money sunk in unexhausted improvements. The Beddington farm, which has now been in operation more or less for twenty years, costs more than this; but if the farm had to be formed now, the experience which has been gained (if it were available, and if members of a local board could condescend to think that anybody else knew what was wanted better than themselves) would enable the board to carry out the work at a much less cost. I say if that experience were available; but, unfortunately, local self-government tends to scatter experience to the winds. Those works, upon which considerable sums of money have been sunk in as yet unexhausted improvements, are not followed up, because present managers do not know anything about them.

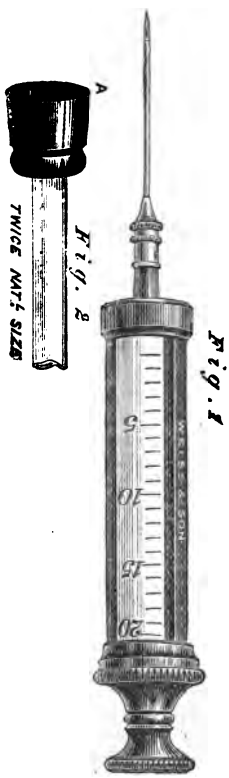
The ninth conclusion to which I have come is, that although sewage should be conveyed to the soil as rapidly as possible, *rainfall* should go to *river*. It is a wrong thing to conduct rainfall into sewers, as by this means a sewage-farm is swamped with unnecessary water. Not that storm-water from the streets of London should be sent into the Thames, or the washings of thickly peopled cities at once into the water-way, but ordinary surface-water should be strictly excluded, and when possible, the rainwater from house-roofs should go to the water-courses. This is a good reason for excluding sewers from thinly peopled districts; ordinary sewers will only drain the subsoil, empty the water-courses, and dry up the smaller sources of our rivers, and produce as much evil in the sub-soil as cesspools now do. They are serious blunders too often perpetrated for the personal benefit of private individuals.

The sixth great law of sanitary work is for sewer and water services to be decidedly separated, so that no interchange of either liquid or gases should be possible. It was shown not long since, that just where a water-pipe passed over a sewer, there the pipe was defective, and as a consequence a serious epidemic arose. Sewage soon decomposes iron; and if water-pipes be allowed to lie in sewers, and to occupy portions of cesspits on the roadside, or to be in communication at those points at which water is delivered into a house and slops and sewage removed,

there can be no real safety for the people. I am sometimes astounded when I hear men who put themselves forward as sanitary authorities utterly ignore this fundamental law.

The seventh law is that the *individual house* is the unit of sanitary work, that it is in each house, and it is with each individual that the first action must be taken to diminish the power of *z* to grow and produce its kind, whilst should *z* be introduced, it is the *individual case* which is the unit of repression. But all these canon laws have their foundation upon the particulate nature of contagia, and it is by bearing this one fact in mind that sanitary work can produce sufficient fruit to be successful.—*British Medical Journal, Oct.* 25, 1879, *p.* 647.

102.—PISTON FOR SYRINGES AND PUMPS.



This is an arrangement patented by Messrs. John Weiss and Sons, which obviates the inconvenience attending the use of syringes fitted with ordinary pistons. No packing is required in the special construction adopted. The end or sucker consists of a cup-shaped or cylindrical disc of vulcanite, which resists the action of liquids, and does not become dry. There is consequently no danger of the end of the piston failing to fit tightly in the tube, and so acting imperfectly. The free edge of the vulcanite disc is very thin, and sufficiently elastic to secure a vacuum. The principle of construction is applicable to pumps of all kinds, and will be readily understood on reference to the accompanying woodcut. Fig. 1 shows an hypodermic syringe of the new pattern, Fig. 2 the piston on a large scale. The instrument submitted to us is of admirable make, and the arrangement described commends itself as likely to attain the full advantages claimed for it. The ingenuity of the contrivance will be appreciated; but the practical point gained is that there will be no danger of leakage, or of the passage of air through any interspace between the piston and the tube. *Lancet, Feb.* 7, 1880, *p.* 214.

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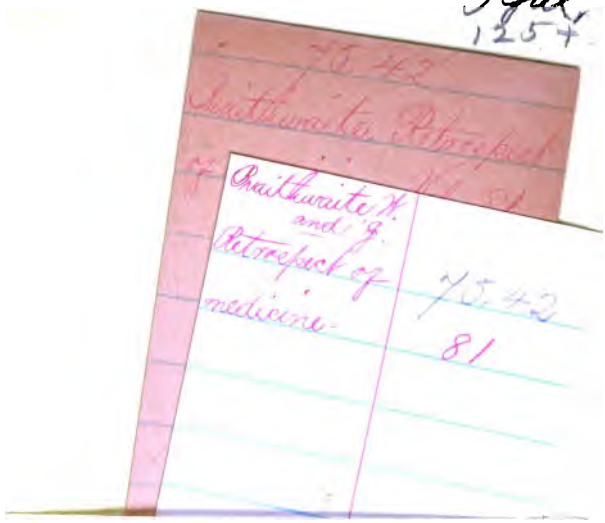
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